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ON THE ESTIMATION OF THE MIDDLE OF LINES

By H. K. WOLFE

The following study was completed and written a few weeks previous to the death of Dr. H. K. Wolfe, late professor of philosophy and psychology at the University of Nebraska. The contribution to the field of psychology, and especially to the field of judgment, seems so great that permission was secured from the wife of the author to have it published. It is with filial devotion to Dr. Wolfe that I, a former student and assistant, present the work for publication.

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HISTORICAL

Nearly three quarters of a century ago E. H. Weber suggested that the just perceptible difference in the length of two lines is a constant fraction of the length of one of the lines; and that generally this fraction is not greater than one twentieth, and may be as small as one hundredth.[1] Weber's experiments were made apparently with both eyes in free movement and at the most favorable distance. The lines to be compared were not shown together; but first one, then the other, was placed in the same field.

F. Hegelmayer,[2] following a similar method, also obtained results in substantial agreement with the psychophysic law, though his experiments were too few for final decision and too incompletely described for comparative use. Hegelmayer sought to determine the effect of small intervals of time upon our memory-images for length of lines (*Gedächtniss für Linearanschauungen*). He prepared on paper seven lines from 15 to 150 mm. long, as given in Table I. He then made other lines, both longer and shorter than these standards, and proceeded according to the method of right and wrong cases in its primitive form. His experiments were very few and apparently very unsystematic. The results given in Table I (H) are a summary of all his experiments. The variable time-element between seeing the standard and making the judgment (5 to 60 sec.) has been neglected in this Table. The numbers denote only the percent. of right cases for each line. Remembering that the differences between the lines to be compared were constant fractions of the length of the lines, the approximate agreement with Weber's law is recognized.

[1] R. Wagner's *Handwörterbuch der Physiologie*, III, ii. 1848, 568.
[2] Vierordt's *Archiv für physikalische Heilkunde*, xi., 1859, 844-853.

Fechner[3] experimented according to the method of average error, by attempting to place the points of a pair of compasses just as far apart as those of another pair of compasses. Both pairs lay on the table side by side, and were covered except at the points.[4] Fechner's problem was therefore the comparison of distances between points rather than the comparison of the lengths of actual lines. Five horizontal distances were used. The results, and approximate distances, may be seen opposite F in Table I. For each length or distance 120 experiments were made with the normal distance on the left, and as many more with the normal distance on the right. On the whole there seems to be no constant difference in Fechner's results obtained in these two positions of the normal distance.[5] These results coincide remarkably well with the requirements of the psychophysic law.

A. W. Volkmann also used the method of average error. However, instead of compass points, he employed vertical white threads against a dark background.[6] Forty-eight experiments with the standard length on the right and an equal number with the standard length on the left were made for each of the lengths 10, 20, 40, 80, 120, 160, 200, 240 mm. Later the whole series, except for the shorter two distances, was repeated. The results of both series may be studied in Table I. (V 1 and V 2). These results agree very well with those obtained by Fechner, indicating both the validity of Weber's law and the indifference of the relative position of the standard length, that is, whether it lies to the right or left of the line with which it is compared. Volkmann found that the proportional increase of the error with that of the normal line apparently does not hold good for very short lines (0.2 to 1.4 mm.). Fechner thought this result might be explained by the relative size of a new constant.[7] Even if an exception, this would merely illustrate the well-known limitations of the law in other sense judgments, viz., that near the threshold and also near the upper limits of sensation the results show greater variations, or the law ceases entirely to express the relation between stimulus and sensation.

In number of experiments and in carefulness of performance these investigations of Fechner and Volkmann are doubtless all that could be desired. The agreement of these two men and Weber regarding the validity of the psychophysic law for linear measurements by the eye was almost enough to deter even the boldest antagonist. In itself the work is not interesting, and much time and patience would be necessary to open the discussion with new data. The remarkable fertility and great extent of the field now called experimental psychology made it still less desirable to work over the ground so well explored by these pioneers. Hence for many years no attempts were made to verify or to disprove their conclusions.

A. Chodin[8] of St. Petersburg finally undertook an elaborate investigation of the validity of Weber's law for eye measurements. He used the method of average error and in part, also, the method of least observable difference. The former method will be understood except where the latter is specifically mentioned. The plan of procedure was as follows: a fine black line was drawn on paper; the standard length was marked off by short cross lines near the middle of the first line; the observer marked off from the cross lines a length equal to the standard line, both on the right and on the left of the standard line or, in case of vertical measurements, above and below the standard, thus giving a standard length between two estimated

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[3] G. T. Fechner, *Elemente der Psychophysik*, i., 1860, 211.
[4] *Ibid.*, 214.
[5] *Ibid.*, 214ff.
[6] Fechner, *Psychophysik*, i., 215ff.
[7] *Ibid.*, 212, 221ff.
[8] Graefe's *Archiv für Ophthalmologie*, xxiii., 1877, 92ff.

TABLE I.

Results of Investigations on Comparing the Lengths of Horizontal Lines: General Summary

Observer	Follow Weber's Law?	Distance of eyes, mm.	No. of judgments each line	Line mm. 2.5	5	10	20	30	40	50	60	80	90	100	120	150	160	200	250	M	V
H.	Yes		90			59		62			62		80		67	70				1.61	.03
F	Yes	350	240			1.61	1.60	1.68	1.61	1.55										1.61	.03
V I	Yes	800	96			1.31	.97		1.34			1.08			1.16		1.28	.96	1.15	1.16	.12
V II	Yes	800	96						.77			.97			1.04		1.00	.98	.94	.95	.06
C I	No	350	150	2.56	1.92	1.50	1.32		1.45			1.37					1.54			1.67	.33
C II	No	350	120	2.00	1.28	1.11	.89		1.07			1.14					1.41			1.27	.26
M I	Yes	600	20			2.50	1.75	1.80	1.75	1.30	1.50	1.25	1.45	1.20	1.60	1.50	1.40	2.50		1.67	.33
M II	Yes	600	20			3.00	2.00	1.70	1.40	1.40	.90	1.80	2.70	3.00	2.70	2.30	2.40	1.90		2.00	.50
M III	Yes	600	20			3.00	4.50	3.30	3.00	2.80	2.70	2.50	2.30	2.00	2.40	2.30	2.30	2.00		2.53	.42
H I	No	500	500			3.24	3.04			1.73				1.88		2.25		2.55	2.73	2.42	.47
H II	No	500	4800							82.1				80.0		78.2		75.8			
F I	Yes	200	80		.66	.59	.64	.64	.67	.90										.68	.07
F II	Yes	200	80		1.50	1.42	1.09	1.11	1.27	1.40										1.30	.14
H.W.		Free	150				.57			.46				.59			.56		.68	.57	.05
K.W.		Free	100				.44			.45				.46			.52		.57	.49	.05
25		Free	250				.89			.73				.78			.77		1.06	.85	.10
25		Free	250				.75			.79				.79			.92		1.22	.89	.14
98		Free	490				1.22			1.17				1.28			1.28		1.62	1.31	.12
88		Free	440				1.22			1.17				1.28			1.36		1.42	1.29	.08

Table I. is a summary of results from all important investigations on this subject. In spite of the varying circumstances under which the experiments were made there is too great similarity in result to warrant the contradictory conclusions drawn by the individual writers.

The first vertical column contains the names of the observers, and Roman numerals designating their several series. The second column gives the answers of the various investigators to the question: Is Weber's Law valid for eye measurements of horizontal lines? The third vertical column shows at which distance the eyes were fixed. The column headed "No. Judgments" shows how many judgments for each length of line are included in the table.

lengths. The standard lengths used were in mm. as follows: 2.5, 5, 10, 20, 40, 80, 160. The distance from the observer's eye to the line was 350 mm. Both eyes were open and were free to move.

Altogether Chodin made about 7000 experiments. He concludes that Weber's law does not hold for eye measurements. Table I. (C 1) gives the results of his first series for horizontal lines, and Table I. (C 2) gives the results of a similar series made later. Comparison of the two series shows the effect of practice. The first series consisted of 150 judgments for each length of line indicated, 75 when the standard was on the right, and 75 when the standard was on the left side of the line to be estimated. The variable error was computed separately for each 15 judgments with a given length. The average of the separate variable errors was taken. The second series contained 60 judgments for each of the two positions of the standard for each length indicated in the table.

The effects of practice are very noticeable, but must not be distorted to support a theory. Chodin says that practice shows greatest changes in the middle distances employed. It is clear from his own figures that practice makes most difference with the results in case of the shortest lines used by him, and that there is a tolerably gradual decrease as the longer lines are reached.

Experiments on horizontal lines by the method of least observable differences were made by Chodin with very similar results. The first series of 120 judgments for each of the seven lengths of lines resulted as follows: 5.88, 3.45, 2.70, 1.89, 2.27, 2.57, 2.33 (in percent. of the total length of lines). The exact repetition of this series gave 3.85, 3.12, 2.22, 1.75, 2.78, 3.12, 3.33. The effect of practice is again seen to be greater on the short lines. Another series of experiments was made by Chodin on horizontal lines according to the method of average error, but differing from the first in having the standard removed before the estimated line is drawn (successive, instead of simultaneous, comparison). In this manner 120 judgments were made for each of the seven standard lengths. The average variable error had the following value in percent. of the length of the line: 4.35, 3.23, 2.70, 2.33, 2.08, 2.00, 2.86. Chodin concludes very positively that Weber's law is not valid in the estimation of horizontal and vertical lines. The relative difference between the standard and the estimated line does not remain constant, but in horizontal lines it decreases from the standard 2.5 mm. long to the standard 20 mm. long, and in vertical lines from 2.5 mm. to 10 mm. long. In both horizontal and vertical lines the relative sensibility for lengths increases thereafter to the standards 160 mm. long. The reason for this difference he finds in the muscular movements of the eyes. When retinal images alone are compared, as in the smallest standards, the larger the image the greater the discrimination, hence the decrease in the relative difference; but when muscular action accompanies the judgment, as in the longest standards, the reverse is true. Comment on this remarkable interpretation is obviously unnecessary. In agreement with other investigators Chodin finds that discrimination for horizontal distances is finer than for vertical distances.

Before leaving the very interesting data given by Chodin I will call attention to a general summary of his results for which I alone am responsible. It is the average of all his measures of discrimination for horizontal lines, however obtained, and must not be used for any other purpose than that of deciding the bearing of his experiments upon the validity of Weber's law for eye measurements. The distances are the same as those already given. He made 630 experiments for each length of standard line, with the following average results: 3.73, 2.60, 2.05, 1.64, 1.93, 2.04, 2.29. Chodin also made 1260 judgments of vertical lines. These results, strange to say, were even more closely in accord with Weber's law than were his experiments with horizontal lines.

Münsterberg's experiments were most carefully planned and executed.[9] In the number of normal distances employed they exceed all others. Owing to the great number of problems investigated and the consequent variation in the conditions only a few experiments can be directly compared with the work of others. In all his experiments the head was fixed at a distance of 600 mm. from the line. He judged by the method of average error the horizontal distance between two sets of two points each, which lay in the same straight line. The two middle points separating the distance to be compared were 60 mm. apart for M 1 and M 2 in Table I. For each normal distance at least twenty single judgments were made, half with the normal distance on the right, and half with it on the left. The results given in Table I opposite M 1 were obtained with the use of both eyes and perfect freedom of eye movement. These may be compared directly with those marked C 1 and C 2. The only difference in method was that Münsterberg used a movable slide to change the length of the line and Chodin used a pencil to mark off his line. Many more experiments for each length of standard were made by Chodin than by Münsterberg. It will be noticed that the mean of the average variable errors is 1.67 in C 1, and also in M 1. In C 2 practice has much reduced both the average error and the variation from the mean. We need not emphasize this absolute agreement between the empirical data of Chodin and Münsterberg to lose ourselves in wonder at the opposite conclusions reached by these investigators. M 2 in Table I. contains results of a similar series of experiments with the use of the right eye only. The general tendency is strikingly similar to that shown when two eyes are used. The only essential difference is the slight increase of the variable error and a corresponding increase of the mean variation of the separate variable errors. M 3 gives results of a similar series with the use of both eyes, but from memory of the normal distance seen just before each experiment. There is again an increase in the mean of the average variable errors, but the mean variation is less than in the preceding series.

These three series representing the results of less than 2000 experiments are about the only original data that the interested reader can obtain from Münsterberg's long article (56 pages) describing 20,000 judgments. The remaining facts given us are mere summaries of numerous valuable tables. These summaries contain just four numbers each, *viz.*, the mean constant error when the standard is at the right, the same error when the standard is at the left, the mean variable error, and the mean variation of the separate variable errors from the average. The reader must accept on faith, not only Münsterberg's conclusions, but also the facts upon which his conclusions are based, and the validity of his reasoning processes. The present writer is at a loss to understand wherein such "experimental psychology" differs from "rational psychology."

Münsterberg's tables bring out a fact mentioned by others in certain particular cases;[10] that "every change in movement, position, or manner of using the eyes manifests itself in the judgments." These influences are often so complex that their specific determination and especially their explanation may be postponed until much additional material shall become accessible. However, it may be urged justly against Münsterberg, I think, that he soon forgets his modest and cautious introduction, wherein are shown the multiplicity and complexity of finely shaded influences which we are unable at present to analyze. A few lines further on he transfers the results obtained solely upon one observer (himself) to all mankind, and proceeds to offer anything but an empirical explanation of the phenomena. Mün-

[9]H. Münsterberg, *Beiträge zur experimentellen Psychologie*, ii., 1889, 125ff.

[10]H. Messer, Poggendorff's *Annalen*, clvii., 1875, 172.

sterberg overestimates a distance at his left compared with one at his right. He then concludes that people in general have this propensity, and explains it by our acquired facility in moving the eyes from left to right in reading.[11]

I find that about twice as many persons overestimate the distance at the right as at the left. Remembering that, in reading, every time the eyes are moved to the right they must be moved back again, and generally are moved back more rapidly and with less attention, *i.e.*, more easily, it occurs to me that this explanation will fit my facts quite as well as the contrary facts of Münsterberg. Those who are unable to accept Münsterberg's conclusions from his experiments can but regret that much of the space occupied by comment was not given to the material itself.

Seldom have so much time, care, skill, and ingenuity been given to work of this kind. Complete tables would have afforded a rich mine for other investigators. The present writer, perhaps, does not differ much from Münsterberg's opinion in regard to the importance of the muscles in space perception; but, because Münsterberg's experiments show that he judges distances more accurately with movable eyes than with fixed eyes, one need not believe "dass alle Grössenschätzung nur Intensitätschätzung der Muskelempfindungen ist." [12]

As a general result of some 20,000 experiments Münsterberg concludes that Weber's law finds one of its best witnesses in eye measurements of extension.

Higier[13] had for his object the determination of the relative value of the two psychophysical methods in eye measurements. His discussion is therefore confined chiefly to the bearing of his results on these methods, and only incidentally touches on the practical truth of Weber's law in the field of visual judgments of spatial relations. He used especially the methods of average error and of right and wrong cases. The observers remained in the dark and compared the lengths of slits, 0.5 mm. wide, in a screen through which more or less light came. Fewer problems were investigated and more experiments were made under the same conditions than was the case with any other investigator of this subject.

The observer's eye was 50 cm. from the line, and for the experiments given in Table I. only the right eye was used. The head was fixed, the eye movable and normally directed towards the inner ends of the two lines to be compared. Perhaps the greatest error that was apparently neglected in these experiments arose from the frequent change of recorders, who, of course, had also to read off the error of observation. As far as the results by the method of average error are concerned we feel even less satisfied with the meagre tables of Higier than with those of Münsterberg. In Table I. (H 1) is given about one-third of the data we are entrusted with, for 3500 experiments, 500 of each distance estimated. It would be interesting and perhaps helpful to know, not only the final average variation, but also the separate averages of each ten or twenty experiments.

During each sitting Higier secured 350 single experiments. Such a number would be impossible or nearly worthless with a less interested observer. On the whole his experiments seem to have been very carefully performed. The normal distance was varied from right to left, the comparative distance was alternately taken too large and too small, and the separate results were tabulated to show the influence of these two factors. In comparing distances with the right eye Higier finds a decided tendency to overestimate the part on the left of the median line. Münsterberg found the same tendency to exist when he used both eyes.

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[11] *Ibid.*, 166-168.

[12] *Ibid.*, 179.

[13] H. Higier, Wundt's *Philosophische Studien*, vii., 1892, 232ff.

In agreement with Higier, Kundt[14] found, in attempting to halve a line 100 mm. long, that his right eye tended to overestimate the left half of the line and his left eye to overestimate the right half, his error being about twice as great in the former case as in the latter. Aubert[15] is said to have made a similar observation.

Higier's experiments by the method of right and wrong cases give results in substantial agreement with those just described. He used only four distances, 50, 100, 150, and 200 mm., for each of which 4800 determinations were made, 480 for each of the ten gradations. Five of these gradations were larger and five smaller than the normal distance; the difference being in both cases 1/100, 2/100, 3/100, 4/100, 5/100, of the normal distance. Half of the experiments were made with the normal distance on the right, and half with it on the left. In half the cases it was larger and in half smaller. On the average about seventeen percent. more correct answers are found when the comparative distance is smaller than the normal distance. The difference is greater when the normal distance lies on the left side. The two methods give results in remarkably close agreement, as far as the sensitiveness depends upon the length of the lines compared. Both methods show that for observer Higier, and for the distances 50, 100, 150, 200 mm., the sensitiveness decreases with increase of the distances to be compared. This decrease is so regular for the distances investigated that one is tempted to look for an undiscovered constant, especially since the two methods agree in this respect. Higier concludes and very positively declares that spatial measurements by the eye do not follow Weber's law.

R. Fisher[16] (Augenartzt in Leipzig) undertook to determine the relative values of distances in four directions from a point. That is, he compared the length of the four arms of a cross whose plane was perpendicular to the line of vision. He used the general method of average error and proceeded in two ways: by attempting to halve both the vertical and the horizontal lines forming the cross, and by attempting in turn to make each of the four arms of the cross equal to a given arm. The variable error in the first case is found to be only about one-half that obtained by the second method. The cross stood at a distance of 200 mm. (?) from the eyes. Only the arms of the cross in use for a particular experiment were in sight. The arms were not marked by visible lines, but by empty spaces between metal finger points. Experiments with all four arms of the cross followed each other regularly, and successive experiments were always of unlike kinds. In the results given in Table I., F 1, both eyes were used and free movements were permitted.[17]

The problem in this case was to divide in the middle the distance between two metal points in the same horizontal line. In order to compare

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[14]A. Kundt, Poggendorff's *Annalen*, cxx., 1863, 135.

[15]H. Aubert, *Physiologie der Netzhaut*, 1865, 263ff.

[16]R. Fischer, Graefe's *Archiv für Ophthalmologie*, xxxvii, i., 1891, 97ff.

[17]The distances used by Fischer do not quite correspond to those in Table I. Below are given the exact distances and the corresponding values of the variable error in percent. of length of line. It is to be observed that the average value for distances 15.9 and 23.8 is given in Table I. under distance 20 mm. and that in the second half the averages of distances 8.7 and 11.5 is given under distance 10 mm.

I.	Length of line, mm.	6	8.7	11.5	16	21.9	31.6	45.2
	Percent V. E.	1.50	1.47	1.37	1.09	1.11	1.27	1.40
II.	Length of line, mm.	12	15.9	23.8	34.2	48.2	68.4	96.0
	Percent V. E.	.66	.58	.60	.64	.64	.67	.90

these results with those obtained by making one line equal to a given line it is evident that one-half the distance must be taken as the basis.

In attempting to make one horizontal arm equal to a given arm Fischer found the variable error about twice as large as in halving a line. The relation of variable error to length of line, however, remains about the same. Table I. (F 2) gives the relations for approximate lengths. Experiments were also made by Fischer with the use of the right eye only. His tables with this condition deal more particularly with the constant error. He states that the variable error is similar but absolutely larger than when two eyes are used, the ratio being as 4 to 3.

Fischer is more interested in the constant error and in the practical results of his experiments,—which is for several reasons unfortunate. In the first place, although he has devoted a good deal of time and care to making a large number of experiments, he has used very short and very inconvenient lines; quite sufficient, perhaps, for his own purpose, but rather unsatisfactory to others. Again, the value of his results on the constant error is probably nearly zero, on account of defects in the observer's own vision. This point, however, will be considered in a section specially devoted to the constant error in eye measurements of horizontal and vertical lines. Fischer concludes that the estimation of lengths of lines follows Weber's law.

While it is true that the results of the various investigations on this subject cannot be used in exact comparison with one another, because of differences in the conditions, yet there is enough general agreement in methods to warrant a superficial comparison in respect to the validity of Weber's law for eye measurements. The uniformity of results revealed is clearly dependent upon a principle, and is not the consequence of self-correcting errors in the various methods of investigation. Table I. gives the results of the several investigators already mentioned. The figures are only approximate, since several have been reduced to the same scale, often from incomplete data.

The variety of methods and conditions precludes any exact quantitative comparison. The wonderful agreement in facts, however, cannot but arrest our attention.

The last part of Table I. contains the results of experiments differing still more in methods and conditions, but nevertheless, as far as general conclusions are concerned, having more or less claim to be compared with those of the first half. Fortunately it does not often happen that conclusions so contradictory are drawn from facts so nearly in harmony. Münsterberg thinks his experiments indicate the validity of Weber's law. The results of Higier conform to the requirements of this law even more nearly than do those of Münsterberg, yet Higier concludes that Weber's law does not hold for such judgments. Weber, Fechner, Volkmann, and Fischer declare for the law, Chodin decides against it. Higier attempts to reconcile his conclusion with that of Wundt by showing that Wundt, Fechner and Volkmann do not recognize the validity of the law for small distances. While this is true, it does not in the least affect the results of Higier, whose shortest distance was 10 mm., which scarcely approaches the minuteness of the distances referred to by Fechner and Wundt.

From a study of these investigations I am led to conclude that the divergence of opinion results chiefly from subjective conditions. In eliminating various subjective factors, those that remain stand out all the more clearly. We need an objective method of interpreting experimental data, or else a little more willingness to wait a while for full explanation of carefully ascertained facts.

The recognition of this fact is clearly seen in the greater attention now given to the communication of detailed methods and results in long tables of figures and pages of description of conditions with short paragraphs of

interpretation. This is, of course, the severest trial for the young psychologist, especially if he be a "born" psychologist. The lesson is sometimes expensive, but many are learning it. The assumption made by a few enthusiastic observers that they are "normal," and hence that any result obtained on themselves may be unconditionally generalized as a law of human nature, is not always concealed by the usual specific disavowal of such intention. It is not given to many to say the final word on any subject, and he who deals with the complex problems of human nature will wisely expect that the final word of today will receive no consideration tomorrow. The evidence apparently indicates that the psychophysic law holds good in eye measurements of distances which we are ordinarily accustomed so to compare. It does not seem to hold for any sense beyond the limits of usual experience, and it is not improbable that the law itself expresses the sum of our *knowledge about anything*, rather than the relations between parts of that sum. In other words, the psychophysic law may express the mean result of *all* our sources of information, supplemented, diminished, and corrected by many avenues of experience, without necessarily expressing the effect of the separate sources of our knowledge. In short, it may be simply the law of normal experience, rather than the "law of the relations between mind and matter." When this normal experience is interrupted by fixing the head, limiting the distance, closing one eye, and so on, the variations become greater. It is very evident that one would not normally compare lines 250 mm. long at the same distance from the eye as lines 5 mm. long. This view is supported by most of the results already attained, and will account for the tendency in later, more refined and isolated methods to produce results differing more from the law and from the earlier results obtained with simpler and more natural methods. This conclusion receives very strong confirmation from my own experiments on the question of ordinary eye measurements.

Experiments at the Nebraska Laboratory

The experiments which I shall now describe were not undertaken to confute or to confirm Weber's law, or even with a view to the solution of any disputed psychological question. Since 1890 my beginning classes have been accustomed to work two hours each week in the laboratory. For several years the classes were small and the laboratory work was very carefully supervised. The experiments now under consideration were all performed in my presence, not more than six students working at a time. All measurements and computations were made by myself. The facts which form the basis of this paper were obtained from several hundred adult students who tried to mark the middle of lines.

As "lines" strips of white paper 1 cm. wide were employed. The observer's estimate of the middle was indicated by making a pinhole with a long glass-headed pin while the strip lay on the table before him, for the horizontal measurements. The observer sat squarely at the table fronting the source of light, so that each end of the strips received about the same illumination. The approximate middle of the line was directly opposite the eye of the observer, whose instructions permitted free movement of the body, head and eyes. Our purpose was to make the

determinations as nearly normal as possible. The instructions were to mark the apparent middle of the lines. The students did not measure the results during the progress of the investigation. They were requested to keep their attention on what was immediately before them, rather than to speculate on what they had just done. Nevertheless, it was found that many of them learned the nature of their prevailing errors before the work was concluded. In most cases, I think the request to mark the *apparent* middle prevented any conscious allowance for discovered tendencies. In no case was I able to find any change in results which could be attributed to this influence. The only restriction upon the duration of a single judgment was that after the pin approached the paper the determination should be completed within 10 sec. Generally less time was used, but as no means of marking this time was employed it occasionally may have exceeded 10 sec. The paper strips were appropriately marked after each judgment in order that the person, the relative position, and the order of the judgment could be determined by the recorder.

The judgments of vertical lines were made under the same conditions. The paper strip was held by the left hand against an upright board, so that the middle was about on a level with the eyes. The distance of the eyes from the paper was usually about the reading distance of the student. For the longer lines there was undoubtedly a tendency to increase this distance; but of course it could not be increased beyond the length of the arm, and this was, in fact, the only restriction placed upon this factor, both in vertical and in horizontal measurements. No tentative marks were permitted. The pin was held in the hand, the point placed on the paper, and moved to right or left (or up or down) until the apparent middle was found. The order of making the judgments was always the same, viz., beginning with the shortest line, one for each length placed horizontally 50, 100, 200, 350, 500 mm.; then one for each of the same lengths held vertically. I call this a double series. Five double series were completed at one sitting, usually occupying nearly an hour, since each strip had to receive the personal and serial marks of the observer, and haste was especially avoided. Five other double series were made, usually about a week after the first set.

The persons taking part in the experiments now under consideration were nearly all advanced students of the University. The others were teachers taking work in psychology. I sought the most favorable conditions for estimating normally the middle of lines, both in the horizontal and in the vertical position. No attempt was made to determine the separate factors. In this case the number of persons investigated is, of course, of more importance than the number of experiments each person makes.

Only in a few cases was there reason to suspect that another set of experiments would have differed much from the one given. At the same time the variation is so great that less than five judgments for any length would have little or no value.

All measurements of errors were made by myself, and recorded to the nearest half mm. Since the ten results of each person for any one length were measured in immediate succession, it was possible to increase the accuracy of measurements beyond that indicated by the individual judgments recorded. Though using the same means as Münsterberg, and working as carefully as possible, I cannot claim that an accuracy of one-tenth mm. was attained. Practically it was found best to fold the papers and to measure from the crease to the pinhole. The material, consisting of some 80,000 judgments, made by more than 800 persons, naturally divides itself into two parts. I shall present first the results of attempts to mark the middle of horizontal lines; but before doing so a word concerning the value to experimental psychology of the statistical method may not be out of place.

No number of unskilled observers can take the place of the trained psychologist. There are open to investigation, however, many questions concerning which structural peculiarities of the individual prevent him from arriving at normal results. In other cases no structural difference can be found, but the results show a personal idiosyncracy that forbids the thought of considering them as standards. This is especially true in our estimation of space relations, whether we consider the immediate comparison of one line with another or the reproduction of space relations from memory. It is also equally true with the relations of both small and large dimensions. As far as horizontal and vertical lines are concerned, many examples of constant errors may be found in my tables. Examples in every-day life, both in space and time, are known to every careful observer.

The facts set forth in Table I. indicate the danger of generalizing without extending the investigation to many persons. Indeed the entire history of the science of experimental psychology, as well as that of all other sciences, shows that men unwillingly learn to be cautious. Perhaps also even the modern psychologist may attach undue importance to results obtained by self-experimentation. Being a "normal" man he may not remember that *other* people are, for the most part, abnormal, and that science cares more about the rest of the world than it does about him. I believe that the sacrifice of accuracy and control by the use of unpractised observers does not render the results worthless, but on the contrary that these more or less crude results may form an important, if not a necessary, supplement to much

of the work of experts. It is therefore with a view of supplementing the patient labors of several admirably equipped special investigators that the following tables are presented.

Experiments with Horizontal Lines

Table II. contains the results of 50 observers, 25 men and 25 women. Every person made ten judgments for each of the following lengths of lines: 50, 100, 200, 350, 500 mm. The observers' names were arranged alphabetically (men first and women last), and numbered as in the first vertical column. The next five vertical columns contain the total average error of ten judgments for each of the five distances, expressed in percent. of the whole length of line. The seventh vertical column contains the arithmetical mean of the five average errors. The eighth column indicates the mean variation of these five, several, average errors from their own mean. The figures in the first horizontal line of the Table indicate the lengths of lines which were to be divided in the middle. For example: the second number in the second horizontal line, 1.20, means that the sum of the errors made by Mr. B. (1 in the first vertical column) in attempting to mark the middle of a line 50 mm. long, divided by the number of judgments and by the length of the line, and this quotient multiplied by 100, is equal to 1.20; $(6\text{x}100) \div (10\text{x}50) = 1.20$. The next four numbers of this line were obtained in a similar way.

Table II. contains the relation of the variable error to the entire length of the line, also expressed in hundredths. As the method of halving a line differs in several important respects from that of making one line equal to another, it may be well to state the manner of obtaining the variable in the present investigation. All the ten judgments of each set were made under the same conditions. A slight difference in the method of obtaining the variable error needs to be explained to render the results intelligible and useful to those who may not understand how the variable error can be obtained by my method of measurement. Only the amount and direction of deviation from the true middle are recorded. Instead, then, of having an average length of line from which to subtract each separate estimated length, we have an average error with a particular sign (*i.e.*, the constant error) which must be subtracted from, or added to, the errors of the separate judgments. It is subtracted when the particular error is on the same side as the average constant error and is greater than the latter; otherwise it is added to the constant error. This is doubtless more difficult to understand, but it is of considerable practical value in measuring the lines.

TABLE II. (Horizontal Lines)
Relation of Total Average Error to Length of Line, Expressed in Hundredths

Line	50 mm.	100	200	350	500	Mean	Average variation from mean	Relation of average variation to mean
Men								
1	1.20	1.20	1.47	1.91	2.30	1.61	.39	1/4.1
2	1.70	1.15	1.20	.84	1.00	1.18	.22	1/5.4
3	1.60	2.60	2.52	1.10	1.38	1.84	.58	1/3.2
4	.40	.70	.70	.76	1.32	.78	.22	1/3.6
5	.50	.85	1.05	.64	1.09	.83	.20	1/4.1
6	1.00	.85	1.05	1.16	2.15	1.24	.36	1/3.4
7	.60	1.00	1.20	1.86	1.55	1.24	.37	1/3.4
8	1.00	1.05	1.45	1.02	.81	1.10	.18	1/6.1
9	.80	.60	1.20	1.27	1.20	1.01	.25	1/4.0
10	.80	.90	.95	.31	1.71	.93	.32	1/2.9
11	.70	.60	.58	.68	.87	.69	.08	1/8.6
12	.70	.70	.90	.63	1.05	.80	.14	1/5.6
13	.90	.90	1.57	1.06	.96	1.08	.20	1/5.4
14	1.50	2.50	4.20	3.58	3.47	3.05	.84	1/3.6
15	1.80	1.95	1.48	1.73	1.50	1.69	.16	1/10.5
16	2.00	2.50	2.05	1.50	1.89	1.99	.23	1/8.6
17	.80	1.10	.63	.64	1.40	.95	.28	1/3.4
18	1.70	.95	.98	.87	.66	1.03	.27	1/4.0
19	.50	.65	.53	.48	1.37	.71	.27	1/2.6
20	1.80	1.65	1.97	1.96	1.50	1.78	.16	1/11.1
21	1.40	1.15	1.15	1.26	1.42	1.28	.11	1/11.7
22	.70	.55	.97	1.73	.95	.98	.30	1/3.3
23	1.20	1.05	1.10	1.07	1.20	1.12	.06	1/18.7
24	2.00	.90	1.05	1.51	1.77	1.45	.38	1/3.9
25	1.10	.75	1.67	2.24	2.20	1.59	.53	1/3.0
Av.	1.14	1.12	1.34	1.28	1.47	1.28	.11	1/11.6
Women								
1	1.30	1.75	1.95	1.20	1.21	1.48	.29	1/5.1
2	.60	.30	.65	.64	.99	.64	.14	1/4.6
3	1.30	.80	.90	.61	1.23	.97	.23	1/4.2
4	2.30	1.45	1.03	1.24	2.05	1.61	.45	1/3.6
5	.40	1.30	.70	.74	1.13	.85	.29	1/2.9
6	1.60	1.65	1.20	.98	1.62	1.41	.26	1/5.4
7	.90	.85	1.90	.53	1.63	1.16	.48	1/2.4
8	2.20	2.35	.75	.78	.75	1.37	.73	1/1.9
9	.50	1.25	.93	1.37	2.41	1.29	.48	1/2.7
10	.90	.95	.88	1.18	2.51	1.29	.49	1/2.6
11	.70	.50	1.17	1.27	1.25	.98	.30	1/3.3
12	1.00	1.55	.63	.97	1.73	1.18	.39	1/3.0
13	.70	1.05	1.13	.89	1.10	.97	.14	1/6.9
14	.80	.75	.68	1.14	2.01	1.08	.40	1/2.7
15	1.10	.80	1.08	1.18	.97	1.03	.11	1/9.3
16	1.20	1.00	1.15	1.27	1.29	1.18	.09	1/13.7
17	.90	.90	1.62	2.43	2.39	1.65	.61	1/2.7
18	2.40	1.30	.78	1.06	1.57	1.42	.45	1/3.2
19	.50	.80	1.20	.77	2.61	1.18	.58	1/2.0
20	1.30	1.15	1.87	1.02	1.98	1.46	.37	1/4.0
21	.60	.85	1.40	1.46	2.27	1.32	.47	1/2.8
22	1.30	.85	1.03	1.70	1.03	1.18	.25	1/4.7
23	.70	.60	.92	2.14	2.16	1.30	.68	1/2.0
24	2.00	1.40	2.20	2.00	1.82	1.88	.22	1/8.6
25	.30	.50	.23	.57	.73	.47	.16	1/2.9
Av.	1.10	1.07	1.12	1.17	1.62	1.22	.36	1/7.6

If one admits the possibility of using the device of halving in the method of average error, there will be little objection to this plan of dealing with the results.

Reserving the discussion of the bearing of these results upon the validity of Weber's law till all the evidence at hand has been presented, it will be worth while to examine some details of the Table. While doing so, however, it must be remembered that with two or three exceptions these results are from untrained observers and that only ten observations were taken for each length of line. *No results from these observers are omitted, and no other observers finished the required work.* There was, of course, no selection of results, but all who attended the class did this work, and both here and in the other tables of this article all acquired data are given as first obtained. In several instances students have repeated the experiments because they did so much worse than their fellows. This was permitted for their own satisfaction, and usually resulted in considerable improvement. Only in one case, however, was this new result substituted for the old. This case does not appear in Table II. or III. The substitution was made in Table IV., because the student was seriously ill at the time the test was made. The repetition showed less than one-fourth as great errors. With the exception of number 14, there is great uniformity among the men in the amount of the total error. The lowest mean for an individual is .69%, and the highest mean is 1.99%. The lowest average for any length of line is .40%, and the highest is 2.60%. The average of all mean judgments is 1.28%, and the average variation from the average is .11% (including number 14). The very large total error of number 14 is due to his great constant error, as is proved by the very moderate size of the variable error. This was the most peculiar case of the entire number of 50 students. Some attempt at a further analysis of this error will be described later. The average variation of the different observers from their own mean is less than we might have expected, it being for these 25 men only .28% of the total length of the line.

Table II. also presents the results obtained by 25 young women under precisely the same circumstances. Here the uniformity is even greater than for the men. The lowest mean for a woman is .47%, the highest 1.88%. The lowest average for a single line is .23%, and the highest 2.61%. The average of all mean judgments is 1.22%. The average variation of the means from this average is .23%. The average variation of the women from their individual means is greater than that of the men, being .36%. As far as any conclusion can be drawn from this fact, it shows that these young women are, as individuals, more variable than the men, but that they differ from each other less than do the men. These figures, however, do not differ much

TABLE III. (Horizontal Lines)

Relation of the Variable Error to the Length of Line, Expressed in Hundredths

Line, mm.	50	100	200	350	500	Mean	Average variation from mean	Relation of average variation to mean
Men								
1	1.20	.90	.94	1.03	1.50	1.11	.19	1/5.8
2	1.10	.65	.93	.84	.90	.88	.11	1/8.0
3	1.44	.94	1.17	.84	.67	1.01	.23	1/4.4
4	.48	.70	.55	.75	1.40	.78	.25	1/3.1
5	.54	.85	.97	.50	1.01	.77	.20	1/3.9
6	1.08	.82	.71	.47	1.33	.88	.26	1/3.4
7	.68	1.00	.90	.96	1.41	.99	.17	1/5.8
8	.80	.46	.48	.83	.73	.66	.15	1/4.4
9	.96	.60	.57	.63	.60	.67	.11	1/6.1
10	.80	.82	.44	.36	1.82	.85	.39	1/2.3
11	.80	.44	.48	.65	.89	.65	.15	1/4.3
12	.80	.50	.90	.61	1.03	.77	.17	1/4.5
13	.76	.40	.81	.63	.91	.70	.16	1/4.4
14	.70	.70	.56	.93	1.01	.78	.15	1/5.2
15	1.32	1.23	.91	1.10	1.28	1.17	.13	1/9.0
16	1.00	.90	.71	.99	.99	.92	.09	1/10.2
17	.48	.60	.63	.51	.54	.55	.05	1/1.1
18	.90	.87	1.03	.90	.69	.88	.08	1/1.1
19	.50	.56	.53	.41	1.39	.68	.29	1/2.4
20	.48	.68	1.13	.62	1.11	.80	.25	1/3.2
21	1.00	.60	.65	.48	.94	.73	.19	1/3.8
22	.74	.55	.73	.96	.69	.73	.09	1/8.1
23	1.28	.96	.74	.84	.82	.93	.15	1/6.2
24	1.20	.74	1.04	1.23	1.67	1.18	.23	1/5.1
25	1.10	.85	1.05	1.24	1.06	1.06	.09	1/11.8
Av.	.89	.73	.78	.77	1.06	.85	.17	1/5.1
Women								
1	.72	1.46	1.40	.94	1.19	1.14	.25	1/4.6
2	.68	.32	.65	.54	.99	.64	.16	1/4.0
3	.58	.42	.73	.61	1.13	.69	.19	1/3.6
4	1.68	1.35	1.00	1.21	1.92	1.43	.29	1/4.9
5	.48	1.10	.72	.78	1.13	.84	.22	1/3.8
6	.68	.82	.50	.90	1.10	.80	.17	1/4.7
7	.54	.60	.92	.60	1.63	.86	.33	1/2.6
8	.64	.61	.65	.79	.72	.68	.06	1/11.3
9	.56	1.16	.69	.95	1.15	.90	.22	1/4.1
10	.36	.85	.63	.99	2.13	.99	.45	1/2.2
11	.42	.42	1.08	1.27	.81	.80	.30	1/2.6
12	.84	1.55	.63	.80	2.03	1.17	.50	1/2.3
13	.70	1.00	.89	.68	1.08	.87	.14	1/6.2
14	.88	.56	.61	1.09	1.02	.83	.20	1/4.2
15	1.10	.80	.63	1.10	.93	.91	.16	1/5.7
16	.80	.50	.80	.55	.56	.64	.13	1/5.0
17	.90	.60	.68	1.27	1.39	.97	.27	1/3.6
18	1.40	.64	.58	.89	1.44	.99	.34	1/2.9
19	.54	.80	.83	.61	1.39	.83	.22	1/3.8
20	1.36	.95	1.04	.88	1.52	1.15	.23	1/5.0
21	.68	.75	.75	.53	.83	.71	.08	1/9.0
22	.78	.65	.93	.99	.59	.79	.14	1/5.6
23	.74	.52	.93	2.02	1.50	1.14	.49	1/2.3
24	.40	.75	1.11	1.39	1.46	1.02	.36	1/2.8
25	.36	.50	.26	.65	.76	.51	.16	1/3.2
Av.	.75	.79	.79	.92	1.22	.90	.24	1/6.4

from those of the first part of the Table. The small differences are generally in favor of more uniformity and keener discrimination on the part of the women.

These tables of total errors show many interesting facts not revealed as clearly by either the variable or the constant error. Why, for example, should the shortest line show a larger percent. of error than the average of all lines? The explanation of this point may also show why several of the women are more influenced by this than are any of the men. Besides the peculiar interest of the tables themselves to one studying this subject, it is necessary to use the total error for comparisons with later results, in which not enough experiments were made by each individual to justify working out the variable error.

Comparing the total error with the variable error, we find that the former is not without value as a measure of the relative sensibility when many persons are employed. Of course it is not accurate; but, as has been already indicated, sometimes approximate results from many persons are of more practical value than more accurate measurements of a few. These two errors are, on the whole, nearly enough parallel to enable us to interpret the tables that contain only the constant and the total errors.

The details of the Table are doubtless sufficiently clear without comment. The decrease in the size of the variable error as compared with the total error is accompanied by a greater increase in the uniformity of the results from individuals. With a mean error of .85% the men show an individual variation of only .14%. The average of the mean variations from their own average for the different lengths is only .17%. The women show an average, mean variable error of .90%, and a variation from this of .16%. The average of the mean variation from their own averages is .24%.

There are several points connected with these averages that require some notice. The small individual variation we must accept as a fact requiring no explanation. People of the same general intelligence, age and mode of life compare lines with about the same results. The results of any one individual for the various lengths of lines need a word here, though the meaning of this equality will be considered later. We observe that these individual variations from the individual average vary, for the men, from .05% to .39%, with an average of .17%; and, for the women, from .06% to .50%, with an average of .24%. At first thought, so small a variation for the average of inexperienced observers, making so few experiments, may seem strange, especially when it is remembered that Münsterberg, with four times as many lines and twice as many experiments for each line, found for himself a corresponding error nearly

one-half larger, viz., .33% (.3 in his table). Again, Münsterberg's special apparatus for making these experiments ought to have enabled him to compare lines more accurately than is possible without special apparatus. It will also be noticed that Münsterberg's average variable error was 1.67% (1.7 in his table), while that of these students was but .87%. We need not seek an explanation of the difference in the effect of Münsterberg's glasses "which competely corrected all defects of his eyes." In the first place, my figures are based on the total length of the lines employed; hence, when compared with experiments in which one line is made equal to a given line, my figures should be doubled. In the second place, we should expect a greater degree of accuracy in halving a line than in making one line equal to another, even when measured on the same standard. That is, it is certainly easier to compare two parts of a line, when both parts may be varied, than to compare them when one part must remain constant. Furthermore, it is supposed that scientific method requires the systematic arrangement of the experiments so that half are made by beginning with a shorter line and half by starting from a line that is clearly longer. This also interrupts the free judgment that is necessary for the accurate comparisons. I am not now discussing the desirability of the accepted method, but am merely calling attention to the fact that the total errors must be larger under this rule than when the very unstable thing called 'judgment' has fewer, or no, restrictions. Fischer found that the error in halving a line was only about one-half the error in attempting to make one line equal to another. Both errors were based on the same length of line, in the one case on half the line to be halved, and in the other case on an equal standard line. Hence it is not strange that Münsterberg made greater errors than my untrained students.

Fischer's experiments in halving shorter lines (10 to 100 mm.) show smaller variable errors that those of Münsterberg. The average variable error by the first method was .68% of the half line. By the second method the average variable error was 1.30% of the whole standard line. Averages of other observers are as follows: Fechner 1.61%, Volkmann 1.05%, Chodin 1.45%. On the whole, the results from my untrained observers compare very favorably with the work of experienced psychologists, both in the absolute size of the variable error and in the variation from the mean.

As already stated, the gross error is not usually considered a measure of discriminative power. Nevertheless, with the data at hand, some more or less important conclusions may be drawn from a comparison of the total errors. The experiments now to be described were performed exactly as those of former classes,

except that only half as many experiments were made. Table IV. contains the summaries of five judgments for each of five horizontal lines by students in psychology for the academic years 1893, '94 and '95. To save space, the Table gives the averages for five students in a single group. As before, the names were written alphabetically, and the divisions formed in order. The three classes were kept separate, and the men were arranged first in each class. At the time the experiments were made these persons had never heard of Weber's law.

Table V. contains a record of similar experiments made by children of the eighth, fourth and first grades in the Lincoln public schools. Instead of arranging the names alphabetically, they were left in the chance order in which the results were first measured amd recorded. Partly on account of lack of time, and partly on account of several sources of error not found in other experiments, only three lengths of lines were given to the children in the public schools.

The training in high school and college apparently does not greatly assist one to measure lines by the eye. Such additional sources of error as are introduced in these grades do not apply to the shortest line, and yet the average error for this line is but little greater for these pupils than for juniors and seniors in the university. The chief source of error, possibly affecting these results, was in the short desk-top on which the lines were judged. The middle of this top may have been marked by some of the pupils, though the inkstand was at one side. The error from this source was not great, as is shown by the results themselves. In the first place, it could affect only the longest line, and the averages for this line have nearly the same relation to those of the other lines as is found among both older and younger observers. In the second place, boys more frequently deface the desks than the girls, yet the boys show no better results in the long lines than do the girls.

Children of the first grade were too immature to understand general directions; hence results from them were obtained only by an individual method. Each child was told what he was to do, in the plainest language, and this was repeated and explained until he understood it. Much time was required to get the smallest children to do the work. They were also very careful and deliberate, thus again using much more time. It was not expected that results from very small children would show great regularity, or have much value. For these reasons little work was attempted with so young children. Only two lengths of lines were used and only two judgments for each line were made by each child. In the Table are given together the averages for

TABLE IV. (Horizontal Lines)

Relation of Total Average Error to Length of Line, Expressed in Hundredths (University Students in Psychology)

	Line, mm.	50	100	200	350	500	M	V	R
Men	Number of Observers								
Class of 1893	5	1.18	1.60	2.20	1.62	1.95	1.71	.29	1/5.9
	5	1.28	1.54	1.14	1.58	1.15	1.34	.18	1/7.4
	5	1.44	1.24	1.04	1.34	1.86	1.38	.21	1/6.6
	5	.96	1.08	1.50	1.78	1.81	1.43	.32	1/4.5
	4	1.55	1.08	1.01	1.26	.93	1.17	.19	1/6.2
Class of 1894	5	1.48	1.68	1.89	1.49	1.35	1.58	.17	1/9.3
	5	1.04	1.10	1.23	.97	.91	1.05	.09	1/11.7
	5	1.04	.86	1.03	1.09	.97	1.00	.07	1/14.3
	5	1.36	1.02	1.26	1.01	.96	1.12	.15	1/7.5
	5	1.20	1.26	1.33	1.57	1.18	1.31	.11	1/11.9
	5	.76	.72	.99	1.01	1.24	.94	.16	1/5.9
Class of 1895	5	1.48	1.28	1.36	1.60	1.82	1.51	.16	1/9.4
	5	.84	.60	.86	.73	.78	.76	.08	1/9.5
	5	1.56	1.54	1.75	1.59	1.93	1.67	.13	1/12.8
	5	1.40	1.14	1.19	1.18	.98	1.18	.09	1/13.1
	5	1.24	1.14	.82	.91	1.20	1.06	.16	1/6.6
	5	1.80	1.16	1.37	1.05	.85	1.25	.27	1/4.6
	5	1.08	1.04	1.03	1.22	1.81	1.24	.23	1/5.4
	5	.84	1.00	1.32	1.54	1.49	1.24	.25	1/5.0
	4	.85	1.38	1.30	1.05	1.28	1.17	.18	1/6.5
	Av.	1.22	1.17	1.28	1.28	1.32	1.25	.17	1/7.4
Women	5	1.16	1.10	1.29	1.10	1.29	1.19	.08	1/14.9
Class of 1893	5	.92	1.14	1.32	1.51	1.49	1.28	.20	1/6.1
	5	1.44	1.30	1.30	1.51	1.46	1.40	.08	1/17.5
	3	1.60	1.10	1.17	.83	1.29	1.20	.20	1/6.0
Class of 1894	5	1.60	1.70	2.11	2.52	2.70	2.13	.39	1/5.5
	5	1.36	1.30	1.30	.58	.79	1.13	.29	1/3.9
	5	.84	1.02	1.00	1.18	1.27	1.06	.13	1/8.2
	5	1.04	1.24	.65	1.18	1.63	1.15	.24	1/4.8
	5	1.12	1.10	1.56	1.70	1.16	1.33	.24	1/5.5
	4	1.08	.72	1.45	1.39	1.72	1.27	.30	1/4.2
	4	1.00	1.33	1.87	1.56	1.93	1.54	.30	1/5.1
Class of 1895	5	1.12	1.24	1.22	1.23	1.58	1.28	.12	1/10.7
	5	1.20	1.06	1.08	1.07	1.49	1.18	.13	1/9.1
	5	1.28	1.06	1.14	1.31	1.58	1.27	.14	1/9.1
	5	1.32	1.10	1.06	1.58	.93	1.20	.20	1/6.0
	5	1.44	1.38	1.72	1.86	.94	1.47	.26	1/5.7
	6	1.07	1.10	.80	1.11	1.06	1.03	.09	1/11.4
	6	1.30	.98	.95	1.25	1.27	1.15	.15	1/7.7
	Av.	1.22	1.17	1.28	1.36	1.42	1.29	.20	1/6.6

TABLE V. (Horizontal Lines)
Relation of Total Average Error to Length of Line, Expressed in Hundredths (School Children)

Line, mm.	Number of observers		50		200		500		Mean	
Eighth	Boys	Girls	Boys	Girls	Boys	Girls	Boys	Girls	Boys	Girls
Grade	5	5	1.08	1.56	.96	2.27	1.54	1.61	1.19	1.81
	5	5	1.80	1.16	.82	1.23	1.15	1.32	1.26	1.24
	5	5	.96	.80	1.11	.52	1.46	1.28	1.18	.87
	4	5	1.30	.92	.82	.89	.89	1.22	1.00	1.01
		5		1.04		1.15		1.65		1.28
		5		1.08		1.25		1.17		1.17
		5		1.80		1.86		1.25		1.64
		6		1.43		1.44		1.28		1.38
		6		1.23		1.79		1.21		1.41
Average			1.29	1.22	.93	1.35	1.26	1.33	1.16	1.31
Fourth	5	5	1.32	1.40	.91	1.09	1.02	2.02	1.08	1.50
Grade	5	5	.96	1.88	1.15	1.20	2.14	1.85	1.42	1.64
	5	5	2.24	2.36	1.35	1.39	1.94	1.78	1.84	1.84
	5	5	1.20	2.40	1.28	1.68	1.75	2.48	1.41	2.19
	5	5	2.44	1.40	1.18	1.47	1.41	1.75	1.68	1.54
	5	5	2.28	1.88	1.55	1.36	1.76	1.42	1.86	1.55
	5	5	1.68	1.16	1.52	1.06	1.91	1.39	1.70	1.20
	5	5	1.98	1.48	1.95	1.77	2.21	1.76	2.05	1.67
	5	5	2.60	2.64	2.26	2.62	2.27	2.11	2.38	2.46
		4		1.92		1.18		3.74		2.28
Average			1.86	1.85	1.46	1.48	1.82	2.03	1.71	1.79
First	5		2.75		2.84				2.80	
Grade	5		2.25		2.37				2.31	
	5		2.04		2.32				2.18	
	5		2.00		2.10				2.05	
	5		2.00		1.56				1.78	
	5		2.42		2.30				2.36	
	5		1.92		1.62				1.77	
	5		2.00		1.81				1.91	
	5		2.25		2.51				2.38	
	5		3.08		2.56				2.82	
Average			2.27		2.20				2.24	

twelve children, arranged in the chance order of original measurements. That is, each number is the average of twenty-four judgments, made by twelve different children.

The increase of the total error is certainly not greater than we should expect. The uniformity is surprisingly great. When it is remembered that no results were omitted, and that the order of arrangement was in no case changed from the chance order in which the results were originally measured and recorded, the constancy of even child-nature receives another witness.

For comparison with these few experiments on many persons I desired to have some results depending on a larger number of

experiments than the general student would have time to make. Several students made additional experiments, but at the time I was most interested in this work few students were giving much attention to psychology. In recent years other problems have engaged our attention, and interested students have been directed to other work. Mrs. Wolfe and myself, however, made many further experiments. Those conducted under the same conditions as have been described are summarised in Table VI. The relation of the variable error to the length of line is presented. Ten judgments for each length of line constitute a series. The series are arranged in the order in which they were made. The effect of practice, therefore, may be easily seen. Usually a half-series was made at one sitting. About half the experiments in this Table were made before any results were measured. In later experiments, of course, we tried to be as objective as possible. Since the constant error in our cases was very small, I do not think that the knowledge gained by measuring the earlier results greatly influenced our judgments. Moreover, the variable error was not worked out till the experiments were finished. This circumstance, however, must be taken at no more than its real value. I am confident that the influence of just such factors is playing a very important part, even in more "exact" investigations than those described in this article.

Practice is seen to have considerable effect only in the case of H. K. W. It seems to manifest itself nearly as clearly in the short lines as in the long ones. The addition of the longer lines (750 and 1000 mm.) produces no perceptible disturbance either in the estimation of the shorter lines or in the final average. Subjectively there was a decided feeling of difference between judging lines 50 mm. long and lines 1000 mm. long. It is probable that the greater ease experienced when judging the shorter lines just after the long is a reason for the negative effect of practice noticed in the short lines of K. B. W. It was certainly very difficult to give the same degree of attention to the comparatively easy task of marking the middle of a line 50 mm. long, that one was unconsciously compelled to give to a line twenty times as long.

The smaller number of experiments in a series is sufficient to explain the rather large variation of the numbers in the body of the Table. However, when the series are taken as units the average is remarkably constant, especially in case of K. B. W., and the variation for the different lengths of lines is exceedingly small. Münsterberg's average variable error was almost exactly five times our average variation. For H. K. W. it is .60%, and for K. B. W. it is .49%. But if we consider the series as units, the average error is for H. K. W. .61% with an average

variation of .14, which is less than one-fourth of the average. The remarkable regularity of the work of K. B. W. is seen in this manner of presenting the data. Her average is .49%; and the average variation from this number is only .10, or about one-fifth of the average variable error.

TABLE VI. (Horizontal Lines)
Relation of Variable Error to Length of Line, Expressed in Hundredths: H.K.W. (man) and K.B.W. (woman)

Line, mm.	50	100	200	350	500	750	1000	Mean		
H.K.W.										
First ten	.74	.55	.72	.96	.67			.73	.10	1/7.3
Second "	.82	.44	.54	.52	1.27			.72	.26	1/2.8
Third "	.56	.64	.78	.83	.62	.70	.57	.67	.08	1/8.4
Fourth "	.56	.38	.70	.50	.72	.61	1.02	.64	.15	1/4.3
Fifth "	.48	.42	.68	.66	.65	.98	1.07	.71	.18	1/4.0
Sixth "	.68	.58	.75	.46	.82	.58	.63	.70	.11	1/6.4
Seventh "	.48	.40	.62	.74	.80	.59	1.25	.70	.20	1/3.5
Eighth "	.48	.40	.71	.71	.50	1.01	.78	.66	.17	1/3.9
Ninth "	.36	.60	.68	.66	.65	.35	.80	.59	.13	1/4.5
Tenth "	.60	.60	.41	.34	.96	.60	1.20	.67	.23	1/2.9
Eleventh"	.62	.45	.54	.42	.61	.38	.66	.53	.09	1/5.9
Twelfth "	.40	.18	.55	.37	.45	.20	.54	.38	.11	1/3.5
Thirteenth	.70	.37	.48	.42	.63	.47	.77	.55	.13	1/4.2
Fourteenth	.54	.38	.55	.30	.48	.53	.52	.47	.08	1/5.9
Fifteenth"	.54	.50	.20	.56	.39	.46	.28	.42	.11	1/3.8
Average	.57	.46	.59	.56	.68	.57	.78	.61	.14	1/4.4
K.B.W.										
First ten	.36	.50	.26	.65	.76			.51	.16	1/3.2
Second "	.32	.40	.45	.44	.50			.42	.05	1/8.4
Third "	.20	.46	.36	.50	.82	.31	.65	.47	.16	1/2.9
Fourth "	.48	.38	.52	.61	.78	.31	.34	.49	.13	1/3.8
Fifth "	.48	.44	.52	.44	.25	.39	.41	.42	.06	1/7.0
Sixth "	.48	.40	.45	.37	.35	.44	.80	.47	.10	1/4.7
Seventh "	.68	.48	.38	.54	.45	.48	.26	.46	.09	1/5.1
Eighth "	.48	.65	.64	.71	.67	.62	.74	.64	.06	1/10.7
Ninth "	.36	.30	.61	.49	.63	.61	.44	.49	.11	1/4.5
Tenth "	.60	.51	.43	.49	.48	.47	.69	.52	.07	1/7.5
Average	.44	.45	.46	.52	.57	.45	.54	.49	.10	1/4.9

Experiments with Vertical Lines

The experiments with vertical lines were made at the same time as those with horizontal lines, and under conditions already described. Although there was another new factor introduced besides the change in position of the lines, I believe the results may have some value by direct comparison with those of the horizontal lines. Since we used paper strips for lines, a difficulty was met when we came to mark a vertical line. The strip would not remain upright without some holder. Convenient holders could easily be obtained, but they disturbed the judgment, if allowed to appear on the paper. Any holder that would

not interfere with the judgment required time to adjust; hence we employed the natural though rather primitive holder that each student was accustomed to use,—the hand. For the short lines I do not believe that this method was very objectionable. For the two longest lines (750 and 1000 mm.) it was at least inconvenient. Since these longer lines were used only by persons much interested in the work, part of the defect was counterbalanced. A special stand with vertical board was necessary for such work, and this precluded investigation in the city schools, without more time than could be given to the subject. The question is one that will repay further investigation, especially upon younger children.

I shall present these Tables in the same order as those dealing with the results of horizontal measurements. Here also I shall defer treating of the theoretical bearing of these facts, and especially their relations to the results of other investigators, and to the psychophysic law.

If we compare Tables VII. and II., the most noticeable difference is the great increase in the average total error. For horizontal lines the average for all lengths, and all persons was: men, 1.28; women, 1.22. For vertical lines this rises to 1.73 for men, and to 1.74 for women. Further experiments show that far the larger part of this increase is due to the judgment of long lines. Indeed, it is true that the total error for very short lines (50 mm.) is less for vertical than for horizontal lines. The increase of error with that of length of line is fairly regular, till the longest line shows more than twice the relative error of the shortest. Such large errors are, of course, some indication of the greater difficulties in the way of these experiments. They are accompanied by very great irregularities in results. With facts of this kind it is clearly useless to take the average variations from the mean result for the separate lengths, except for the sake of showing uniformity, or the lack of uniformity, among individuals of the general standard of age and intelligence. However, even more interesting ways of comparing individuals are found in the mean of all individuals for any one length of line, and in the average variation from this mean. Comparison made in this manner shows very little difference between the results from vertical and horizontal lines; and yet the individual variations are enormous. Even the greatest relative difference is found with the longest lines.

Men and women show a strange agreement for three of the five lengths. In the shortest line the women are better than the men, and in the next to the longest line the men make a smaller error. The average for all lines and all persons is almost exactly the same for men and women (men, 1.73; women, 1.74). The greatest and also the least errors are made by men. Number

TABLE VII. (Vertical Lines)
Relation of the Total Average Error to Length of Line, Expressed in Hundredths (University Students in Psychology)

Line, mm.	50	100	200	350	500			
Men								
1	1.30	1.45	2.52	1.97	3.01	2.05	.57	1/3.6
2	.90	.85	.58	.30	1.14	.75	.25	1/3.0
3	1.60	1.55	2.30	1.40	1.86	1.74	.27	1/6.4
4	1.10	1.70	2.32	2.64	3.16	2.18	.63	1/3.5
5	.60	.60	1.05	1.16	2.39	1.16	.49	1/2.4
6	1.80	.80	2.72	3.31	2.85	2.30	.80	1/2.9
7	2.90	3.70	2.17	3.36	2.44	2.91	.49	1/5.9
8	.80	1.30	2.55	2.15	2.34	1.83	.62	1/3.0
9	1.20	2.20	2.10	2.11	1.54	1.83	.37	1/5.0
10	1.20	1.65	1.27	1.53	1.60	1.45	.17	1/8.5
11	.90	.80	1.50	1.23	1.95	1.28	.36	1/3.6
12	1.00	.90	.68	1.40	1.17	1.03	.20	1/5.1
13	.40	.80	.87	.86	1.78	.94	.33	1/2.8
14	.70	1.05	1.75	.80	1.40	1.14	.35	1/3.3
15	2.10	1.85	1.60	1.63	3.00	2.04	.41	1/5.0
16	.90	1.60	2.67	3.86	4.89	2.78	1.27	1/2.2
17	1.20	.40	1.40	.93	1.90	1.17	.40	1/2.9
18	2.60	2.40	3.12	4.30	3.63	3.21	.60	1/5.4
19	1.20	1.55	2.52	2.37	1.95	1.92	.43	1/4.5
20	.80	.50	1.05	2.27	6.53	2.23	1.74	1/1.3
21	1.00	.95	1.10	.96	1.62	1.13	.20	1/5.7
22	.30	.30	1.02	1.44	1.49	.91	.49	1/1.8
23	1.20	2.25	2.00	2.20	3.03	2.14	.43	1/5.0
24	.70	1.10	2.52	2.26	3.76	2.07	.93	1/2.2
25	.60	.45	.82	1.10	2.34	1.06	.53	1/2.0
Av.	1.16	1.31	1.77	1.90	2.51	1.73	.40	1/4.3
Women								
1	1.50	2.05	3.25	2.41	1.96	2.23	.48	1/4.6
2	1.10	2.55	4.27	2.67	2.96	2.71	.72	1/3.8
3	1.10	.95	1.72	2.26	3.57	1.92	.80	1/2.4
4	.80	1.80	1.90	1.69	2.16	1.67	.35	1/4.8
5	.40	1.05	1.15	1.23	1.16	1.00	.24	1/4.2
6	.70	1.75	2.55	2.31	2.31	1.92	.56	1/3.4
7	1.10	.70	1.20	2.43	3.30	1.75	.90	1/1.9
8	1.50	1.95	1.92	1.74	1.98	1.82	.16	1/11.4
9	1.50	1.25	2.10	3.43	3.95	2.45	1.00	1/2.5
10	.50	.90	.87	1.44	2.65	1.27	.62	1/2.0
11	.60	.40	.97	.50	1.24	.74	.29	1/2.6
12	.70	1.85	2.97	3.66	5.02	2.84	1.25	1/2.3
13	1.10	1.95	1.40	2.71	3.45	2.12	.75	1/2.8
14	.40	.80	1.10	1.47	2.89	1.33	.68	1/2.0
15	2.20	1.05	1.67	2.03	2.95	1.98	.50	1/4.0
16	.80	.85	1.85	1.56	2.37	1.49	.53	1/2.8
17	1.20	.60	1.10	2.53	1.05	1.30	.50	1/2.6
18	.90	.80	2.95	2.99	1.35	1.80	.94	1/1.9
19	1.40	2.75	2.66	3.00	3.28	2.61	.48	1/5.4
20	2.30	.90	1.67	2.29	3.87	2.21	.74	1/3.0
21	.80	.85	1.40	2.67	4.18	1.98	1.16	1/1.7
22	.90	1.70	.72	1.39	1.47	1.24	.34	1/3.6
23	2.10	1.00	1.77	1.20	1.94	1.60	.40	1/4.0
24	.80	1.10	.75	.84	.78	.85	.50	1/1.7
25	.20	.10	.62	1.73	1.13	.76	.54	1/1.4
Av.	1.06	1.27	1.78	2.09	2.52	1.74	.46	1/3.8

14, a man, who made astonishingly large errors on horizontal lines, has, in this table, an error very much below the average. His especial friend, number 19, a man, who had a rather large error with small variation in horizontal lines, has here the largest error in the whole Table, and a variation larger than the average error of number 2. I point out these details to show that one must be careful in generalizing from a few facts, even in such relatively simple processes as judgment of the middle of lines. The men compared above were perhaps more interested in psychology and in experimental work than any others in the class. Upon my advice, one continued his work in experimental psychology in another university, and the other studied medicine and became an oculist.

The variable error shows a similar trend, though the increase for the longer lines is less prominent. The averages for men and women are almost exactly the same. The immense constant error in vertical measurements leaves us a surprisingly small variable error. It is, however, about 14% larger than the corresponding error for horizontal lines; though the greater part of this increase falls to the longer lines. The comparison of judgments of different individuals for the same length of line is also, in this part of the Table, more suggestive than the variation of the same individual for different lines. The Table furnishes data for both kinds of comparison. There is most difference between individuals in the shortest lines, and least difference in the longest lines. Proportionally this relation is greatly increased, since, in absolute size, these two errors are just the reverse of this order. Here again the women are more constant than the men. Their average variation from the mean of all is not much less than that of the men; there are fewer large variations, both in case of different lengths of lines, and in case of different individuals.

This Table, in connection with Tables II. and III., indicates that it is slightly more difficult to mark the middle of short vertical lines than of short horizontal lines, and that this difference increases as the line grows longer, but not in the same proportion. It must not be forgotten, however, that the methods of holding the paper strips made it more inconvenient to mark the long lines. While this doubtless produced its chief effect upon the constant error, it probably influenced also the variable error by making the observer more or less uncomfortable.

Table VIII. contains the relation of the total average error to the length of the vertical lines as estimated by university students of the classes of 1893, '94, and '95. Though only five judgments were taken for each length of line, these results may

TABLE VIII. (Vertical Lines)

Relation of Total Average Error to Length of Line, Expressed in Hundredths (University Students)

	Line, mm.	50	100	200	350	500	M	V	R
Men	Number of observers								
Class	5	1.20	1.52	1.75	2.26	3.25	2.00	.61	1/3.3
of	5	1.40	1.52	2.05	1.87	2.78	1.92	.39	1/4.9
1893	5	1.28	1.44	1.48	2.07	2.88	1.83	.52	1/3.5
	5	1.12	1.50	1.42	2.07	3.18	1.86	.61	1/3.1
	4	1.30	1.15	1.35	1.67	2.07	1.51	.29	1/5.2
Class	5	1.28	1.42	1.88	2.61	2.78	1.99	.56	1/3.6
of	5	.96	1.56	1.28	2.10	2.48	1.68	.49	1/3.4
1894	5	1.40	1.12	1.11	1.20	1.57	1.28	.16	1/8.0
	5	.80	1.04	1.21	1.10	1.19	1.07	.12	1/8.9
	5	1.16	1.14	1.47	1.77	1.89	1.49	.28	1/5.3
	5	.92	1.06	1.18	1.21	1.29	1.13	.11	1/10.3
Class	5	1.04	1.26	1.71	1.25	1.63	1.38	.23	1/6.0
of	5	1.36	1.30	1.03	1.50	1.79	1.40	.20	1/7.0
1895	5	1.16	.86	1.63	2.02	1.86	1.51	.40	1/3.8
	5	1.12	1.22	1.47	1.71	1.90	1.48	.26	1/5.7
	5	1.32	1.42	1.80	1.38	1.99	1.58	.25	1/6.3
	5	1.64	1.40	1.53	2.13	2.16	1.77	.30	1/5.9
	5	1.08	.92	1.24	1.63	1.80	1.33	.30	1/4.4
	5	1.56	1.58	1.21	1.63	2.32	1.66	.26	1/6.4
	4	1.10	1.50	1.88	1.80	2.79	1.81	.42	1/4.3
	Average	1.21	1.30	1.48	1.75	2.18	1.58	.30	1/5.3
Women	5	1.20	.94	1.99	2.00	2.72	1.77	.56	1/3.2
Class	5	.88	1.20	1.50	1.33	2.42	1.47	.40	1/3.7
of	5	1.64	1.52	1.45	1.56	2.52	1.74	.31	1/5.6
1893	3	1.47	1.07	1.88	1.78	3.36	1.91	.58	1/3.3
Class	5	1.12	1.64	1.71	2.20	3.16	1 97	.57	1/3.5
of	5	1.04	.66	1.35	1.86	2.68	1.52	.60	1/2.5
1894	5	1.08	1.46	1.15	2.32	2.68	1.74	.61	1/2.9
	5	1.40	1.52	1.66	1.79	2.08	1.69	.20	1/8.5
	5	1.32	.90	1.45	1.05	1.30	1.20	.18	1/6.7
	4	.88	1.18	1.48	1.60	1.87	1.40	.30	1/4.7
	4	1.20	1.37	1.20	1.31	2.14	1.44	.20	1/7.2
Class	5	1.48	1.08	1.25	2.49	2.66	1.79	.63	1/2.8
of	5	1.88	1.72	1.04	1.43	1.96	1.61	.30	1/5.4
1895	5	1.20	1.28	1.73	1.20	1.66	1.41	.22	1/6.4
	5	1.20	1.34	1.96	2.94	2.80	2.05	.66	1/3.1
	5	1.52	1.20	1.49	1.89	2.04	1.63	.27	1/6.0
	6	1.60	1.37	1.99	1.65	1.96	1.71	.21	1/8.1
	6	1.27	1.33	1.55	1.52	2.05	1.54	.20	1/7.7
	Average	1.30	1.27	1.55	1.77	2.34	1.65	.33	1/5.0

TABLE IX. (Vertical Lines)

Relation of the Variable Error to Length of Line, Expressed in Hundredths (University Students in Psychology)

Line, mm.	50	100	200	350	500	M	V	R
Men								
1	.90	.95	1.19	.84	1.33	1.04	.17	1/6.1
2	.96	.85	.58	.28	.92	.72	.23	1/3.1
3	1.24	.86	1.02	1.47	1.81	1.28	.29	1/4.4
4	1.10	.98	.81	.50	1.38	.95	.24	1/4.0
5	.76	.60	.68	1.16	1.59	.96	.33	1/2.9
6	1.80	1.40	1.95	1.05	1.54	1.56	.26	1/6.0
7	1.48	.56	1.38	2.16	.78	1.27	.48	1/2.7
8	.96	1.14	.60	.51	1.17	.88	.26	1/3.4
9	.64	1.04	.72	1.29	1.01	.94	.21	1/4.5
10	.64	.92	1.10	1.13	1.68	1.09	.25	1/4.4
11	1.18	.62	.59	1.14	1.19	.94	.27	1/3.5
12	.40	.96	.68	.85	1.25	.83	.23	1/3.6
13	.48	.66	.73	.72	1.49	.82	.25	1/3.3
14	.74	.67	1.41	.63	1.40	.97	.35	1/2.8
15	.94	1.60	1.30	1.00	1.82	1.33	.30	1/4.4
16	.90	.70	1.24	1.31	2.57	1.34	.49	1/2.7
17	.88	.40	1.41	.93	1.90	1.10	.44	1/2.5
18	.80	.88	1.38	1.01	1.12	1.04	.17	1/6.1
19	.48	.47	.97	1.42	1.00	.87	.31	1/2.8
20	.88	.58	.96	.56	1.00	.80	.18	1/4.4
21	.80	.85	.71	.61	.68	.73	.08	1/9.1
22	.36	.34	.83	1.03	1.05	.72	.30	1/2.4
23	.68	.68	.60	1.10	1.24	.86	.25	1/3.4
24	.82	1.18	1.25	1.83	1.53	1.32	.29	1/4.6
25	.70	.55	.78	.70	1.02	.75	.12	1/6.3
Av.	.86	.82	.99	1.01	1.34	1.00	.27	1/3.7
Women								
1	1.68	1.95	1.30	1.01	1.02	1.39	.34	1/4.1
2	.56	.77	.53	.71	1.57	.83	.30	1/2.8
3	.56	.70	.73	.69	1.23	.78	.18	1/4.3
4	.88	1.08	1.25	.70	.76	.93	.18	1/5.2
5	.48	.95	1.09	1.24	1.20	.99	.22	1/4.5
6	.42	.60	.78	1.30	1.43	.91	.37	1/2.5
7	1.12	.62	.87	1.28	1.48	1.07	.26	1/4.1
8	.70	.90	1.26	1.40	1.94	1.24	.35	1/3.5
9	.90	.70	.65	1.69	1.42	1.07	.39	1/2.7
10	.56	.84	.78	1.24	1.27	.94	.25	1/3.8
11	.72	.40	.83	.35	1.20	.70	.26	1/2.5
12	.80	1.28	1.67	1.46	1.10	1.26	.25	1/5.0
13	.94	1.34	1.08	.79	1.04	1.04	.14	1/7.4
14	.56	.70	1.05	1.16	1.89	1.07	.36	1/3.0
15	1.04	.66	1.40	.91	1.46	1.09	.27	1/4.0
16	.80	.70	1.20	.70	1.40	.96	.27	1/3.6
17	.90	.62	.85	1.16	1.05	.92	.15	1/6.1
18	1.10	.56	1.74	1.27	1.22	1.18	.28	1/4.2
19	1.00	.95	1.15	.67	1.93	1.14	.32	1/3.6
20	1.90	.62	1.26	1.46	1.63	1.37	.35	1/3.9
21	.80	.75	.95	1.10	1.38	1.00	.20	1/5.0
22	.54	1.12	.58	.96	1.39	.92	.29	1/3.2
23	.90	.68	1.75	1.22	1.68	1.25	.38	1/3.3
24	.40	1.10	.75	.83	.75	.77	.16	1/4.8
25	.20	.16	.44	.92	.48	.44	.21	1/2.1
Av.	.82	.83	1.04	1.05	1.32	1.01	.27	1/3.74

be directly compared with the first part of Table VII. Their value is less than that of other classes only because of fewer judgments. The same time and care were given, both in making the experiments, and in measuring and tabulating the results.

In general these results resemble those already presented. There is a decided increase in the relative total error for the longer lines. Except in case of the longest line as judged by the women this increase is comparatively regular. The men rather excel the women in regularity of judgment, and also in the smaller size of the errors. The two sexes differ, however, only in the slightest degree. Comparing Table VII. and Table VIII. we observe that, while the errors of the earlier classes were smaller for the short lines, they were larger for the long lines than those made by later classes. The larger number of experiments represented by each number in Table VIII., together with the grouping of several persons, tends to make the results of these groups more uniform than the results of the individuals represented in Table VII. I do not know why the results with the longest lines should show less total error in Table VIII. than in Table VII. Two possible reasons suggest themselves, but I cannot measure the influence of either. First, students may, perhaps, have less interest in such work after a first sitting. Secondly, with later classes, a pin slightly longer and more convenient than that used by the early classes was employed.

TABLE X. (Vertical Lines)

Relation of Variable Error to Length of Line, Expressed in Hundredths (H.K.W. and K.B.W.)

Line, mm.	50	100	200	350	500	750	1000	M	V	R
H.K.W.(man)										
First ten	.36	.34	.84	1.00	1.05			.72	.29	1/2.4
Second "	.49	.82	1.00	.74	.34			.68	.21	1/3.2
Third "	.28	.58	.74	1.11	.38	.94	.60	.66	.23	1/2.9
Fourth "	.56	.38	.70	.50	.72	.87	.42	.59	.15	1/3.9
Fifth "	.48	.41	.68	.66	.63	1.73	.68	.75	.28	1/2.7
Average	.43	.51	.79	.80	.62	1.18	.57	.70	.23	1/3.04
K.B.W.(woman)										
First ten	.20	.16	.44	.76	.47			.41	.18	1/2.3
Second "	.18	.30	.58	.50	.69			.45	.17	1/2.6
Third "	.20	.24	.25	.57	.50	.27	.28	.33	.12	1/2.8
Fourth "	.48	.34	.43	.66	.43	.47	.53	.48	.07	1/6.9
Fifth "	.49	.41	.83	.44	.43	.56	.58	.54	.11	1/4.9
Average	.31	.29	.56	.59	.50	.40	.46	.44	.13	1/3.38

Table X. contains the corresponding results from the two observers, H. K. W. and K. B. W., again grouped in series of ten experiments for each length of line. Although not as many judgments were taken as for the horizontal lines, there are

enough facts to form an interesting supplement to Table VI. It is also seen here more clearly than elsewhere in this paper how great is the factor of personal interest, and how small the effect of practice, in work of this kind. The last three groups for each observer were made long after the first five, and are not to be compared with the judgments of horizontal lines of the corresponding group. Itwill be noticed also in judgments of vertical lines that the improvement with practice is confined exclusively to the longer lines. Indeed, there is a decided tendency to neglect the shorter lines in later experiments.

The large error in case of H. K. W. on length 750 mm. remains without explanation. "It just happened that way." A consciousness of failure to do justice to that particular line was present and, perhaps, contributed to the unfortunate result. This state of mind did not arise from knowledge of results obtained by measurements, for these lines were not measured until the experiments were finished. Especially in the last group of ten a sense of failure to approximate the middle of that line grew ever stronger. Though the results of H. K. W. show a very much smaller average error than the corresponding work of students, the errors are, perhaps, no smaller than great interest might account for. The work of K. B. W. with vertical lines, and indeed with horizontal lines, must be considered very unusual. Nevertheless, it is less questionable than any other series in this paper.

As already stated, it was found impracticable to obtain judgments of the middle of vertical lines from school children. An attempt to do so under somewhat different conditions was made for one line. A strip of paper, similar to those already described, 100 mm. long, was placed upon the slightly sloping desk at right angles to the position occupied by the horizontal lines. That is, the paper lay with one end toward the child and with the other end extending away from him. Since the children inclined their heads over the desk while making their judgment, this position would not differ greatly from the normal relation of eyes and line with the method of actual vertical lines. Little objection could be made to this modification, perhaps, for the shorter lines, but it would be very difficult to follow this plan with lines 500 mm. long. For this reason only one length of line was used, and from this method the results at hand are not tabulated with the other experiments. They are given merely to show that approximately the same relations between horizontal and vertical judgments exist as in case of older persons. The groups, corresponding to those given in Table IV., each consisting of five children and twenty-five judgments, yield for the "vertical" line, 100 mm. long, the following percentages of total average error:

Eighth grade: Boys	1.48,	1.28,	2.36,	2.17	Mean, 1.82		
Eighth grade: Girls	1.76,	1.52,	1.20,	1.14,	1.14,	1.26,	1.48
	1.50,	1.80	Mean	1.42			
Fourth grade: Boys	1.96,	2.36,	1.80,	1.58,	2.24,	2.40,	
	2.34,	2.38,	3.47	Mean	2.28		
Fourth grade: Girls	1.66,	1.02,	1.48,	2.04,	1.62,	2.20	
	1.40,	1.84,	1.48,	1.48	Mean	1.62	

Comparison with other results must be made with full recognition of the difference in methods. Conclusions from these numbers must be drawn with great caution.

The Constant Error

One other set of facts is yet to be offered. The constant error may be studied in the following Tables, which are presented in the same order as has been adopted in the preceding exposition. First is given the relation of the constant error of the length of the horizontal line, for the 50 students who made ten judgments for each line. Table XI. shows this relation for the algebraic sum of the errors made by each observer, and whether the error lies on the right or on the left. An error on the right means that in trying to mark the middle of a line the point was placed at the right of the true middle, *i.e.*, the right half was overestimated. For convenience, errors on the right are placed in the column headed R. Errors on the left are placed in the column marked L.

A careful inspection of the details and summary in this Table ought to prevent, in the future, such remarkable generalizations as we find in several monographs based on many experiments upon one individual. The Table will also explain in part the contradictory results obtained by different observers, and improve our appreciation of their facts as well as diminish our confidence in the conflicting interpretations of different sets of facts. Even the psychologist sometimes forgets that experimental data may differ without any fault of the observers, but that a given cause always produces the same effect.

If we are not very conservative, a decided conviction will now force itself upon us. The Table at once shows that the error on the left is more frequent in the short lines than in the long lines. In case of the women, it is found about twice as often in the short lines. When these women judge the longest lines this relation is just reversed. The same tendency is seen in the first half of the Table, though the men do not indicate as great difference. The summary of the first half of the Table shows an average constant error always on the right, but increasing much faster than the length of the lines. Beginning with .36%, it jumps immediately to .61%; then to .88%, near which it remains for the next line also; then falls to .72%. Per-

TABLE XI. (Horizontal Lines)

Relation of Constant Error to Length of Line, Expressed in Hundredths (University Students in Psychology)

Line, mm.	50		100		200		350		500		Mean		mv
Men	L	R	L	R	L	R	L	R	L	R	L	R	
1		.40		1.00		1.18		1.91		2.20		1.34	.57
2		1.50		1.15		1.28		.70	.48			.83	.58
3		.80		2.60		2.48		1.14		1.38		1.68	.69
4	.20		.50		.30			.08	.70		.32		.32
5		.10		.05		.40	.41			.71		.17	.31
6		.20		.85	.00	.00		1.16		1.91		.82	.58
7		.20		.10	.95		.80		.63		.42		.45
8		.10		1.05		1.45		1.14		.53		.85	.43
9		.40	.00	.00		.95		1.27		1.12		.75	.44
10		.80		.60		.95		.15		.67		.63	.21
11		.50		.40		.38		.34	.19			.29	.19
12	.50			.50		.20		.09	.09			.04	.27
13		.70		.90		1.58		.80		.24		.84	.32
14		1.50		2.50		4.20		3.59		3.47		3.05	.84
15		1.40		1.95		1.38		1.36		.92		1.40	.32
16		2.00		2.50		2.05		1.44		1.87		1.97	.25
17	.80		1.00			.23		.59		1.36		.08	.78
18	1.30		.55		.28			.27		.08	.36		.46
19	.50		.45			.38	.49		.11		.23		.30
20		1.80		1.65		1.78		1.96		.74		1.59	.34
21	1.00		1.15		1.00		1.26		1.26		1.13		.11
22		.10		.10		.98		1.73		.59		.70	.52
23		.40		.45		1.10		.81		.94		.74	.25
24		2.00		.70		.05		1.14		.59		.88	.54
25	.70		.15			1.58		2.24		2.18		1.03	.16
Av.	.71	.83	.63	1.06	.63	1.23	.74	1.14	.49	1.19	.49	.98	.44
Mean		.36		.61		.88		.84		.72		.69	.16
Women													
1		1.10		1.45		1.62		1.11		.39		1.13	.32
2		.40		.10	.20			.23	.09			.09	.19
3	1.30		.60		.78		.27		.55		.70		.27
4	1.90		.75		.03		.67			.33	.60		.60
5		.40		.70		.05	.20		.07			.18	.30
6	1.60		1.65		1.05		.30		1.30		1.18		.40
7	.90		.75		1.90		.33			.37	.70		.58
8	2.20		2.35		.50		.43			.45	1.01		1.02
9	.30		.45			.58		1.23		2.01		.61	.80
10	.90		.65		.63			.71		1.01	.09		.76
11		.70		.40		.78		.44		1.21		.71	.23
12		.80		.75		.38		.94		1.09		.79	.18
13	.30			.25		1.05.	46		.24			.06	.47
14	.20		.55		.43			.20		1.97		.20	.71
15	.50		.00	.00	.93		.27			.41	.26		.37
16		1.00		1.00		1.05		1.27		.57		.98	.16
17		.90		1.00		1.63		1.73		2.35		1.54	.65
18	2.50		1.30			.58		.71	.65		.63		1.02
19	.10		.20		1.15		.77		2.41		.93		.68
20		.30	.85		1.48			.76	.90		.43		.77
21	.40		.25			1.40		1.46		2.21		.88	.97
22	1.30		.75			.63		1.36		1.03		.19	.98
23	.10		.60			.33	.63		1.52		.50		.50
24		2.00		1.55		2.20		1.80		1.70		1.85	.20
25	.10		.20		.05			.40	.25		.04		.18
Av.	.91	.84	.79	.80	.76	.94	.43	.96	.80	1.14	.59	.71	.58
Mean	.28		.19			.13		.40		.36		.09	.25

haps even this great difference, on account of its irregularity, would not excite much interest, were it not for the greater, though similar tendency seen in the second half of the Table. The women begin with a constant error on the left amounting to .28%. This decreases slightly for the next longer lines, but still is decidedly on the left, .19%. The next line shows the small error of .13% on the right. The next line increases this to .40%. It then falls to .36%. The series thus presents an extreme variation of .68%, which is not very different from the variation in case of the men. The same relation is discovered if we consider the average errors of those making R and L errors for the different lines. In this case, we divide the sum of the positive errors by the number making such errors, instead of dividing by the total number of observers. For the men the following results are obtained:

R	.76%	1.08%	1.24%	1.14%	1.19%
L	.88%	.64%	.52%	.74%	.49%

The corresponding figures for the women are as follows:

R	.84%	.80%	.94%	.96%	1.14%
L	.90%	.79%	.76%	.43%	.80%

It is, however, not safe to draw conclusions from these facts without remembering the method of conducting the experiments. We have also some further material to examine.

Tables XII. and XIII. contain the relations of the constant error for those later classes of university students who made fewer experiments, and for children of the public schools. The grouping is the same as in the Tables already given. While the irregularity of these separate results is considerable, whether we observe the differences caused by persons, or by lengths of lines, there are certain unexpected uniformities. It might well be supposed that a constant error would indicate a disposition to overestimate, or to underestimate, one half of a horizontal line without reference to the length of the line. We enjoy finding easy physiological explanations of these idiosyncrasies. Often these happy hits are more or less successful. For example, one of my students persistently made a larger constant error at the right (overestimated the right half of the line). More varied and numerous experiments with head fixed, each eye separately, different positions, etc., showed conclusively that this was caused in part by a lameness of the muscles of the right side of the neck, including perhaps to some extent the muscles of the right eye. Furthermore, few people have two eyes even approximately equal in power. The better eye will always do more than half of the work, especially when the task is particularly delicate.

TABLE XII. (Horisontal Lines)

Relation of Constant Error to Length of Line, Expressed in Hundredths (University Students in Psychology)

Line, mm.	50		100		200		350		500		Mean	
Men	L	R	L	R	L	R	L	R	L	R	L	R
1893												
5		.64		1.30		1.26		.83		.93	.99	.23
5	.72		.06			.56		.98		.56	.26	.52
5	.64		.36		.06			.70		1.21	.17	.63
5	.24			.24		.60		1.00		1.10	.54	.43
4		.05		.13		.26		.71		.51	.35	.25
1894												
5	.28		.40			.25		.29		.67	.11	.36
5		.24		.90		.97		.75		.45	.64	.26
5		.28		.74		.93		.61	.23		.47	.35
5		.32		.66		.90		.62		.47	.59	.16
5		.88		.74		1.07		.91		.45	.81	.17
5		.28		.12		.54		.52		.74	.44	.19
1895												
5		1.00		1.12		1.24		1.45		1.65	1.29	.21
5		.12		.28		.60		.20	.01		.24	.16
5		1.24		.94		1.29		1.25		1.25	1.19	.10
5		.52		1.02		1.07		.83		.50	.79	.22
5		.12	.18			.02		.28		.76	.20	.26
5		1.24		.40		.39		.27	.05		.45	.32
5		.60	.30			.37		.74		1.33	.55	.41
5		.60		.52		1.18		1.31		.89	.90	.28
4		.05		.53		.54		.64		.70	.49	.18
Av.	.47	.51	.26	.64	.06	.74	.00	.75	.10	.83	.00 .58	.28
Mean		.32		.42		.70		.75		.69	.58	.16
Women 1893												
5	.36		.34		.15		.39		.50		.35	.08
5	.04		.30		.10		.17		.30		.18	.09
5	.80		.80		.78		.90		.82		.82	.03
3		.80		.10	.17			.27		.35	.27	.24
1894												
5	1.12		.60		.38		.35		.58		.61	.21
5		.44		.44		.04		.06	.08		.18	.21
5		.20		.58		.79		.79		.86	.64	.20
5	.24		.72		.05			.06		.42	.11	.30
5	.88			.86		1.48		1.11		.73	1.01	.23
4		.33		.07		.87		.88		.92	.61	.33
4	.04		.16			1.05		.54		1.28	.53	.51
1895												
5	.24		.00	.00	.34			.11		.61	.03	.27
5		.80		.10		.54		.59		.80	.57	.20
5		.04	.58		.04			.40		.32	.03	.27
5		.16		.02		.55		1.13		1.19	.61	.44
5		.56		.22		1.06		1.41		.71	.79	.35
6		.20		.67		.42		.71		.64	.53	.17
6	.30			.12		.43		.43		.65	.27	.28
Av.	.39	.44	.50	.32	.25	.72	.45	.61	.46	.73	.41 .47	.25
Mean		.07				.29		.37		.40	.22	.16

TABLE XIII.

Relation of Constant Error to Length of Line, Expressed in Hundredths (School Children)

Line		50		200		500		Mean	
	No.	L	R	L	R	L	R	L	R
Boys, Eighth Grade	5		.04		.38		.91		.44
	5	.04			.18		.76		.30
	5	.48			.79		.64		.33
	4	.00	.00		.06		.27		.11
Girls, Eighth Grade	5	1.16		.55		.00	.00	.57	
	5	.84		.53		.27		.55	
	5	.64		.20		.56		.47	
	5		.04	.79		.54		.43	
	5	.48		.51		.42		.47	
	5	.52			.35		.52		.12
	5	1.24		1.38		.50		1.07	
	6	.92		.50			.48	.31	
	6	.77		1.23			.07	.64	
Girls, Mean	47	.73		.59		.14		.49	
Boys, Mean	19	.12			.35		.65		.29
Boys, Fourth Grade	5	.68			.23	.38		.28	
	5		.24		.37	1.32		.24	
	5	1.12		.08			1.32		.04
	5	.00	.00	1.06			.14	.31	
	5	1.64		.50			.20	.65	
	5	.68			.63		1.40		.45
	5	1.12		.42		.54		.69	
	5	.68			.97	.02			.09
	5	.35		.29			1.27		.21
Boys, Mean	45	.67		.02			.23	.15	
Girls, Fourth Grade	5	.44		.67		.09		.40	
	5	1.00		.30			.60	.23	
	5	2.08		.95			.29	.91	
	5	1.84		.66			.61	.63	
	5	1.24		.13			1.57		.07
	5	1.32		.72			.14	.63	
	5	.28			.84	.70		.05	
	5	.76			.25		1.09		.19
	5	.36			.34		1.28		.42
	4		.10	.03			1.40		.52
Girls, Mean	49	.93		.20			.63	.17	
Boys and Girls First Grade	5	.08			.78				
	5		1.50		.16				
	5		.63		.61				
	5		.58		.67				
	5	.17		.15					
	5		1.08		1.61				
	5	.50			.75				
	5	.17		.21					
	5		.25		2.07				
	5		1.08		.13				
Boys and Girls, Mean	50		.42		.64				

This fact does not prove that eye measurements are not chiefly muscle measurements, though I think it weakens much of the evidence that has been adduced in recent times. It is comparatively a simple matter to make experiments with one eye closed, thus shutting off the influence of the retinal action in the closed eye. Results obtained in this manner differ from results obtained with the two eyes open. The muscular movements must have been the same in both cases; hence it is the retinal impression that causes the difference. We need not accept the extreme implication of this conclusion to grant that retinal impressions are necessary accompaniments of muscular sensations in the finer measurements of visual space. It is not a question of the original spatial sense. The eye is principally a *substitute* sense. The retina, so small and so delicately constructed, with sensitive elements so minute and near together, may be called the vernier or the micrometer sense. On the other hand it is also a prophetic, or even a trail-blazing sense.

Tables XIV. and XV. show the constant errors for H. K. W. and K. B. W. Little study of the Tables is necessary to show several important facts, some of which will be still further emphasized in the Tables of constant errors with vertical lines. The most valuable point for experimental psychology seems to me to be the fact, already referred to, that conclusions based on even very numerous experiments from one individual may be misleading. Table X. furnishes material that should make us more cautious about concluding universal laws from experiments upon one individual. What a host of varying conclusions could be drawn from separate individuals of that Table! In many cases a very much greater number of experiments would doubtless change the results, but in numerous cases tested by additional experiments the cause was found to be in the individual, and often the results from two individuals were directly contradictory. Compare number 14 with number 21. The one has a large constant error at the right, increasing with the length of the line; the other has a larger constant error at the left increasing with the length of the line. Compare these two with number 24, whose error is on the left in short lines, grows less, then changes to the right and increases with the length of the lines. Compare all these with number 2, whose constant error just reverses that of number 24. Similar series may be formed from the second half of this Table; indeed, the women show a greater lack of uniformity than the men in their constant errors. We have only to bring together the results of 24, 6, 19, 17, 8, 25, to see that each person is largely a law to himself.

The personal equation in work of this kind will be found to show a greater range than in those observations whose practical importance has compelled its exact determination. Many ele-

TABLE XIV. (Horizontal Lines)

Relation of Constant Error to Length of Line, Expressed in Hundredths: H.K.W. (man), and K.B.(womanW.)

Line, mm.		50		100		200		350		500		750		1000		Mean		Av. Variation from Mean
		L	R	L	R	L	R	L	R	L	R	L	R	L	R	L	R	
H.K.W.																		
First	ten		.10		.05		.98		1.73		.59						.69	.53
Second	"	.30		.20			.73		1.36		.61						.44	.55
Third	"	.10		.45			.65		.91		.89		1.27		.94		.59	.49
Fourth	"	.30		.15		.00	.00		.17		.48		.68		.09		.14	.26
Fifth	"	.20		.60			.08		.26		.81		.57		.23		.16	.35
Sixth	"		.20	.15		.00	.00	.54			.65		.33		.50		.14	.32
Seventh	"		.40		.25	.10			.54		.87		.87		1.10		.56	.33
Eighth	"		.60		.30	.65			.21	.45			.17		.99		.17	.41
Ninth	"		.90		.20	.43		.03			.03	.09		.41			.02	.30
Tenth	"		.50	.00	.00	.58		.37		.60		.20			.39	.12		.36
Eleventh	"		.70	.15		.23		.13		.69		.05			.81		.04	.41
Twelfth	"	.00	.00	.55		.20		.37		.74			.45	.28		.24		.28
Thirteenth	"		.30	.45		.63		.11		.51			.09		.45	.12		.33
Fourteenth	"		.10		.35	.25		.06		.90			.20		.52	.01		.34
Fifteenth	"		.10	.00	.00	.60			.01	.45			.22		.17	.08		.26
Mean			.20	.10		.09			.25		.04		.35		.35		.14	.16
K.B.W.																		
First	"	.10		.20		.05			.40	.25						.04		.18
Second	"	.20		.20			.38	.07		.42						.10		.21
Third	"	.00	.00	.45			.05		.41	.00	.00		.23		.39		.09	.22
Fourth	"		.20	.15			.35	.07		.53		.04			.26		.01	.23
Fifth	"		.20	.20			.20		.21	.17			.09		.08		.06	.14
Sixth	"		.80		.50		.33		.09	.11		.09			.03		.22	.28
Seventh	"		.40		.35		.13		.53		.39	.25		.64			.13	.33
Eighth	"		.40		.75		.66		.91		.41		.34		.22		.53	.21
Ninth	"	.10		.00	.00		.33		.36		.95		.07	1.41			.03	.46
Tenth	"		.40		.15	.03			.60		.28		.11	.60			.13	.26
Mean			.20		.06		.24		.34		.06		.05	.17			.11	.13

TABLE XV. (Vertical Lines)

Relation of Constant Error to Length of Line, Expressed in Hundredths: H.K.W. (man), and K.B.W. (woman)

Line, mm.	50		100		200		350		500		750		1000		Mean		Av. Variation from Mean
	U	O	U	O	U	O	U	O	U	O	U	O	U	O	U	O	
H.K.W.																	
First ten	.10		.20		.83			1.07		.95						.18	.67
Second "		2.30		1.40		1.75		1.37		2.88						1.94	.52
Third "		.20		.20		.60		.99		1.38		1.70		2.66		1.10	.69
Fourth "		.50	.25			.45		.74		.90		1.13		2.96		.92	.64
Fifth "		1.00		1.10		.45		1.64		.11		3.52		1.96		1.40	.84
Average	.10	1.00	.23	.90	.83	.81	.00	1.16	.00	1.24	.00	2.12	.00	2.53		1.11	.67
Mean		.78		.45		.48		1.16		1.24		2.12		2.53		1.25	.61
K.B.W.																	
First ten	.00	.00		.10		.53		1.64		1.13						.68	.56
Second "	.10		.20		.38			.76		.88						.19	.50
Third "		.20	.10		.00	.00		.89		.42		.40		.70		.36	.28
Fourth "	.40		.60		.18			.39		.61		.75		.58		.16	.45
Fifth "	.30		.15		.23			.23		.77		.86		1.29		.35	.53
Average	.27	.20	.26	.10	.26	.53	.00	.72	.00	.76	.00	.67	.00	.86	.00	.35	.46
Mean	.12		.19		.05			.72		.76		.67		.86		.38	.43

ments constitute the cause of this variation of individuals. Some of them are dependent upon the organs of vision and their accessories; others depend upon more general asymmetry of the two sides of the body; others again upon some temporary factor whose explanation I cannot attempt to give.

In illustration of the above points some facts not shown in the Tables may be mentioned, though only a very few can be given place. Certain individuals will have an error regularly at the right for every length except one, and that one shows an error on the left every time. Here the conditions are all alike except length of line, and the different lines are taken in regular order from shortest to longest, only one judgment being made for any one line. When the error is small no one is surprised, but often the error is considerable on each side; and when the refractory line occurs in the middle of the series, as 200 mm. or 250 mm., there is more wonder than explanation. The effect of first judgments on succeeding ones is well known. This explanation seems scarcely sufficient to account for the facts as just described, though we do not understand the influence of subconscious states. A somewhat similar phenomenon was sometimes observed. Each student made five double series at one sitting. The remaining five double series were made a week later. In general these two sets agreed very well. Some individuals, however, reversed the direction of their constant errors at this second trial. When this error was large and uniform at each sitting, but on different sides of the true middle, desire for explanation arose. Sometimes only one length gave a large and uniform constant error at the first sitting. This error fell on the other side the next week, just as large, and with the same irregularity in the other results. This could be easily explained if the students knew the results of the first work, before beginning the next. Since this was not the case we must seek a solution in other ways. The facts suggest that the length of line exerts a peculiar influence upon some observers, when judging in regular order the middle of lines differing in length. What part, if any, of this result is due to the effect of the preceding judgment of the shorter line, or to the unconscious influence of the first judgment of the line having the same length, cannot now be determined. It is probable that these two factors are more efficient than another which may suggest itself, viz., that in the free movement of the head and eyes each length of line will require activity of certain muscles in definite proportions, and different lines will require the service of other muscles, or of the same muscles in different proportions. The change from one week to another would then be explained by the different physical condition of the observer. However this may be, I am chiefly interested in pointing out the complexity

of the process, the difference between observers, and the personal variations of the same observer. Although any observer may show a surprising uniformity in his results for one sitting, at the next sitting he may differ more from his former work than he does from the work of some other observer. Unlike the deviations in the personal equation noticed in the simple time-relations of mental phenomena, these differences are not in proportion to the subjective estimate of relative ability, or of similar physical condition. A feeling of indisposition nearly always produces great change in the variable error; but often such a change, and other changes, are found when the observer feels equally able and alert.

The Tables do not show these details, and it must not be understood that there is no constancy in results from the same individual. Many persons manifest great uniformity in their two sittings. In a large number of sittings, however, even with trained observers, we may expect considerable variation in results obtained on different days accompanied by great uniformity in the series completed in a single short sitting. The Tables showing the relation of the constant errors may be studied for general tendency of the error, although the results, except in the first table, are not separate for the different individuals.

Before extending these results to the human race under all conditions we must remember that the middle of strips of paper was marked by a pinhole made with a pin in the hand of the observer. When a large number of persons mark the middle of strips in the manner described, there is on the average a slight tendency to overestimate the part on the left of the true middle for the short lines, and a greater tendency to overestimate the part on the right of the true middle for the long lines. Both these tendencies are much greater in case of the women than in case of the men. Whether any part of this error is due to the use of the hand and its interference with vision, the present data are insufficient to determine. It is proposed to test this point in the classes of the next few years by varying the method of work. There were too few left-handed persons in the classes examined to furnish even a hint of the proper solution. Those observed, however, did not contradict the general tendency, and those showing the greatest deviation from this tendency were not left-handed persons.

The following Tables furnished the corresponding data for the vertical lines. All averages falling above the true middle are indicated by the letter "O," and all averages falling below the true middle by the letter "U." The individual variations are less interesting than in the case of horizontal lines, though it is again clear that the personal element cannot be neglected in similar investigations. While it is true that nearly all observers

TABLE XVI. (Vertical Lines)

Relation of Constant Error to Length of Line, Expressed in Hundredths

Line, mm.	50		100		200		350		500		Mean		mv
Men	U	O	U	O	U	O	U	O	U	O	U	O	
1		1.30		1.45		2.43		1.97		2.91		2.01	.53
2	.30		.45		.13			.10		1.23		.09	.46
3		1.20		1.55		2.20		.34		.40		1.14	.61
4		.50		1.70		2.33		2.65		3.12		2.06	.77
5	.40			.20		.85	.07			1.91		.50	.71
6		1.20		1.20		1.83		3.31		2.77		2.06	.78
7		2.90		3.70		1.68		3.19		2.44		2.78	.58
8		.40		1.30		2.55		2.14		2.34		1.75	.72
9		1.20		2.20		2.10		2.03		1.44		1.79	.38
10		1.20		1.65		1.13		1.00		1.00		1.20	.18
11		.70		.60		1.40		.54		1.91		1.03	.50
12	.00	.00		.30	.18		1.20		.33		.28		.39
13		.20		.80		.58		.47		1.30		.67	.30
14	.10			.95		1.45		.66		.46		.68	.41
15		2.10		1.10		1.20		1.60		2.80		1.76	.55
16		.90		1.60		2.43		3.86		4.87		2.73	1.31
17		.80		.40		1.35		.93		1.90		1.08	.44
18		2.60		2.40		2.88		4.30		3.63		3.16	.64
19		1.20		1.55		2.53		2.37		1.95		1.92	.44
20		.40		.20		.80		2.27		4.57		1.65	.42
21		1.00		.75		.95		.79		1.60		1.02	.23
22	.10		.20		.83			1.07		.95		.18	.67
23		1.20		2.25		2.00		1.97		2.93		2.07	.42
24	.30			.90		2.53		1.86		3.76		1.75	.16
25		.60		.25		.53		.81		2.34		.91	.58
Average	.24	1.14	.33	1.26	.38	1.72	.64	1.75	.33	2.27	.28	1.50	.61
Mean		.82		1.13		1.46		1.56		2.17		1.43	.36
Women													
1		.90		1.75		3.28		2.41		1.88		2.04	.64
2		1.10		2.55		4.28		2.67		2.96		2.71	.73
3		1.10		.75		1.73		2.26		3.56		1.88	.82
4		.40		1.80		1.85		1.57		2.16		1.56	.46
5		.20		.65		.30	.13			.22		.25	.18
6		.70		1.75		2.55		1.94		2.21		1.83	.48
7	.10			.40		.90		2.34		3.30		1.37	1.16
8		1.50		1.85		1.58		1.63		1.42		1.60	.12
9		1.50		1.25		2.10		2.74		3.85		2.29	.81
10	.30			.70		.43	.41			2.65		.61	.85
11	.60			.40		.78	.47		.84		.15		.59
12		.50		1.85		2.98		3.54		5.02		2.78	1.28
13		.50		1.95		1.30		2.71		3.45		1.98	.88
14		.40		.60		.75		1.16		2.89		1.16	.69
15		2.20		1.05		1.48		2.03		2.95		1.94	.54
16		.40		.75		1.50		1.56		1.93		1.23	.52
17	1.20			.10		.75		2.47		.83		.59	.91
18	.50			.70		2.95		2.99		1.23		1.47	1.20
19		1.00		2.75		2.63		2.87		3.28		2.51	.60
20		1.90		.90		1.03		1.97		3.87		1.93	.79
21		.60		.85		1.35		2.67		4.18		1.93	1.20
22		.90		1.60	.33		.99			.71		.38	.83
23		1.90		.80		.90	.11			1.12		.93	.47
24	.80		.80		.15			.07	.16		.37		.35
25	.00	.00		.10		.53		1.64		1.13		.68	.56
Average	.58	.98	.80	1.16	.24	1.65	.42	2.16	.50	2.47	.26	1.55	.71
Mean		.57		1.08		1.50		1.65		2.23		1.41	.46

have a large constant error caused by overestimating the upper part of the vertical line, it is found that some individuals generally underestimate that part of the line. As far as my data may be taken for normal and sufficient, the proportion between the first and second classes is as ten to one for men, and as thirteen to one for women. The Tables given do not indicate quite this proportion on account of the grouping of the later classes, instead of considering each individual separately as I have done in making the above estimate. I have also taken into consideration the amount of the error as well as the number of errors. Indeed, if number of errors alone be considered, the relation of "O" to "U" judgments is about four to one.

These Tables show certain general facts that have not been brought out by the work of others, and other results that confirm or contradict the conclusions of certain investigators. Our present information is not sufficient to justify a quantitative treatment of the relations between judgments of horizontal and vertical lines. The individual, or personal, element is so powerful that it is at present impossible to determine to what extent the varying, and even contradictory, results of would-be-quantitative investigators depend on the method employed, and to what extent the physical or mental idiosyncrasy of the observer is responsible. Thus far investigators have arrived at results that sometimes agree and sometimes are contradictory. They employ different methods, and account therewith for the contradictions, while the agreements are taken for universally valid laws. It would not be difficult to lay down the theoretical conditions that are necessary for a satisfactory solution of this question. These Tables, however, add not a little to our information on this subject, and explain some of the contradictions existing between the conclusions of some specialists. They have one necessary element not possessed by those of any other observer. The individual was varied, perhaps, quite sufficiently. They lack any variation in method.

These Tables indicate a very strong tendency on the part of most observers to overestimate the upper half of a vertical line. This tendency exists for the shortest lines used, but increases very much more rapidly than the length of the line increases. This same effect of longer lines is seen in case of those who either underestimate the upper half, or have a small constant error. That is, the proportion of such errors to length of line either remains about the same or actually decreases. The women show an error of "U" about twice as large as that of the men. It will be remembered that Fischer devoted his attention very largely to the determination of the constant error. His results are so peculiar that a word concerning the matter seems called for. Fischer attempted to make the four arms of a cross equal to

TABLE XVII. (Vertical Lines)

Relation of Constant Error to Length of Line, Expressed in Hundredths (University Students in Psychology)

Line, mm	50		100		200		350		500		Mean		mv
Men	U	O	U	O	U	O	U	O	U	O	U	O	
1893													
5		.40		.92		1.50		1.17		3.13		1.42	.71
5		.60		.48		1.13		1.30		1.99		1.10	.45
5		.80		1.24		1.08		1.39		2.45		1.39	.42
3		.96		1.42		1.25		1.57		3.06		1.65	.56
4		.58		.32		.66		.78		1.50		.77	.30
1894													
5	.44			.74		1.60		1.66		1.74		1.06	.73
5	.24			.80		1.06		1.83		2.24		1.10	.71
3		1.24		.64		.67		.56		1.10		.84	.26
5	.24			.20		.17		.77		.30		.24	.24
5		.12		.30		1.37		1.78		1.38		1.09	.76
5		.44		.90		1.14		.90		1.04		.88	.18
1895													
5	.16			.26		.51		.72		.92		.45	.32
5		.80		.62		.47		1.06		.86		.76	.17
5		.36		.62		1.39		1.59		1.35		1.06	.46
5		.48		1.10		1.35		1.57		1.42		1.18	.32
5		.76		.78		1.54		.92		1.39		1.08	.31
5		.92		.56		1.23		1.89		1.68		1.26	.42
5	.52		.20		.42			.26		.53	.07		.37
5		1.08		1.54		.99		1.11		1.91		1.33	.32
4	.40		.10			.83		1.41		1.46		.64	.71
Av.	.33	.68	.15	.75	.42	1.05	.00	1.20	.00	1.60	.07	1.02	.43
Mean		.38		.66		.98		1.20		1.60		.96	.39
Women													
1893													
5		.48		.54		1.73		1.80		2.10		1.33	.66
5	.00	.00		.52		1.22		.97		1.72		.89	.50
5	.12			1.20		1.35		1.42		1.00		.97	.44
3		.28		.60		1.07		.99		1.92		.95	.45
1894													
5		.64		1.36		.90		1.82		3.00		1.56	.68
5		.16		.06		1.01		1.79		2.45		1.09	.82
5		.04		.82		.75		1.90		2.66		1.03	.68
5		1.16		1.28		1.54		1.50		1.86		1.47	.20
5		.52		.58		.69		.39		.86		.61	.13
4		.48		.78		1.24		.22		1.61		.87	.45
4		1.07		.83		1.03		.81		1.75		1.10	.26
1895													
5		1.08		.76		.93		1.75		2.26		1.36	.52
5		.80		1.56		.80		1.15		1.62		1.19	.38
5		.88		.76		1.35		.58		1.46		1.01	.32
5		.64		1.10		1.88		2.63		2.42		1.73	.69
5		.80		.44		.83		1.37		1.13		.91	.27
6		1.13		.93		1.31		1.51		1.26		1.23	.16
6		1.00		.45		.73		1.31		1.88		1.07	.42
Av.	.12	.69		.87		1.14		1.33		1.83		1.13	.45
Mean		.61		.87		1.14		1.33		1.83		1.16	.34

each other, and found that, in order to have the vertical arms seem equal to each other, the lower arm must be to the upper arm as 100 to 106.7 (by another method as 100 to 103.22). My Tables show conclusively that this proportion depends largely upon the length of the lines, and that instead of requiring the lower arm to be shorter most persons require it to be longer. The enormous size of Fischer's constant error for the short lines employed is inexplicable. In horizontal lines, Fischer finds very little constant error. I think that the constant error in comparing horizontal lines by means of both eyes may be explained by peculiarities in observer or in method. Fischer states finally that, in order that the four arms of a cross may appear to be equal, they should have the following relations: under arm 100, over arm 103.22, left arm 114.49, right arm 115.44.

The many attempts to express quantitatively the above relation seem to me to have failed, and for the very simple reason that one or both of the most important factors have been neglected. From the facts presented in the Tables of this article it is already clear that heretofore the person and the length of line have not been varied sufficiently; also that this is a problem requiring special conditions for its satisfactory solution.

Conclusions

The theoretical discussion of the facts presented in this paper, and in other papers of similar purpose, is of much interest to the speculative mind. The material is now so abundant that great self-control is required to keep within the limits of scientific conclusions. I have considered only one phase of the measurement of space by the eyes, namely the estimation of the middle of lines. All facts have worth, and while it is true that some facts have greater worth than others, yet it does not follow that the value is directly proportional to the plasticity of the facts. It is very probable that the wealth of material already accumulated on the subject of eye measurements of space relations may have important bearing on some of the greater questions of mind. But it is at least equally probable that this bearing will not be clearly understood until much adjacent territory shall have been explored. Speculations are not harmful in themselves. On the contrary, they are a necessary means of normal progress. There are, however, different kinds of speculation. The legitimate sort are of good family and are helpful when we need them. Another kind sometimes masquerades under various names for the sake of good fellowship, or from worse motives. It is this bastard speculation, bearing the name of exact science, that just now is threatening the welfare of Experimental Psychology.

Before attempting to summarise the results of this investigation some notice should be taken of their bearing upon the psychophysic law. It will be remembered that conflicting decisions have been rendered on this question, and these too from data in substantial agreement. The importance of the question, doubtless, has been exaggerated, but in its present condition worse than dissatisfaction may be caused. As already suggested, this disagreement is not due so much to variation in data, as to subjective conditions in interpretation. The lack of even an approximate standard is plainly manifest. The theoretical determination of such a standard will probably soon be attempted. Meantime several factors of discord may be recognised. It is plain that the results of more recent experiments deviate more from the psychophysic law than do the results of the earlier investigators. These latter clearly followed Weber's law with marvellous fidelity. Some of the later writers have deemed equal exactness necessary to the validity of the law. They have been unable to attain nearly the same degree of approximation; hence have declared against the law. The facts are that none of these investigators approached either Fechner or Volkmann in scientific training or in power of observation. We, therefore, should not expect equal uniformity in result. Furthermore, it is by no means certain that, by the methods employed, the constant errors have been eliminated. Again, the number of experiments made by some of the writers is entirely too small to justify independent conclusions contradicting the results of more extended researches. These considerations, taken in connection with the fact that only a very few individuals were experimented upon, and that several of these subjects were decidedly abnormal in eyesight (even though "the glasses completely corrected the deficiency"), may account for the not great deviation from the requirements of Weber's law.

Referring to Table I., we observe that the chief deviation from a uniform percentage of error is found in the shortest and longest lines of the separate series. I am unable to account for this fact, which is not new, except through the presence of a constant error. In my own investigation no attempt was made to eliminate the constant error that might possibly arise from the order of making the experiment. In the work of all other investigators this error was thought to have been eliminated by alternately beginning with the longest and shortest lines. It is evident that such procedure might possibly divide the error to some extent between the longest and shortest lines; or, if the error of the first judgment of a series could be shown to be opposite in form to the error of the latest judgment in the same series, this method might partially neutralize these errors. For such experiments as mine this hypothesis has no basis. Indeed,

my own observations lead me to doubt its correctness in many kinds of psychophysical experiments. It would not be difficult to show that more frequently than not the greatest variations in a series occur in the extreme members of the series. In other instances, where this is not true, there is often another circumstance which is evidently of similar origin. It is the larger value of the error for the first and last of the series; or better, perhaps, the smaller value of the other members of the series *near* the extremes. This circumstance is not confined to my own experiments, or to experiments on this general subject. It may be observed in very many other Tables where series of observations have been made without changing the order of the series or by merely reversing the order.

In Table I. we notice that the first member of the series is larger than the second in thirteen instances, and smaller in four instances (omitting the series of right and wrong cases). The last member of the series is larger than the one before in thirteen instances and smaller in four instances. In every instance but three the average of the extremes is greater than the average of all the members of the series. Two of these instances occur in the work of the splendidly endowed observers, Fechner and Volkmann, and the third instance shows only a slight excess on the opposite side.

In this Table the order in each series was regularly reversed, except in my own experiments. Had the order been reversed in these, the last member might possibly have been somewhat smaller, and hence the results would have corresponded more nearly with the requirements of Weber's law. In view of the fact that in no experiments have the conditions been varied sufficiently to eliminate the constant error, the deviations from Weber's law, as shown by Table I., do not seem to me to be greater than may justly be expected.

Summary

The following conclusions seem to me to be warranted.

(1) It is possible for even unpractised persons to estimate the middle of horizontal lines not longer than 500 mm. with an average total error of but little over one percent. of the entire line (about 1/80).

(2) On the average practice may reduce this error about one quarter. Individuals without practice differ more from each other than do practised persons; yet some persons judge better at first than others are able to do after considerable practice.

(3) Vertical lines not longer than 1000 mm. are judged equally well with horizontal lines. For longer vertical lines the relative error increases, perhaps, a quarter of one per cent. with

each 100 mm. increase in length (up to the limit of my lines, 500 mm.). But with trained observers this increase is not so great, and in individual cases may almost disappear, even without conscious allowance for the discovered tendency.

(4) Age produces less effect on the judgment of the middle of lines than might be expected. The total error of adults may be given as 1/80, and eighth-grade children are quite equal to adults. Fourth-grade children have an error of about 1/60. First-grade children show an error of about 1/40 for lines not longer than 200 mm.

(5) On the average there is practically no difference in the ability of men and women to mark the middle of horizontal lines. In the case of vertical lines a slight advantage seems to exist in favor of the men. Contrary to well-known speculations, the women seem to differ more from each other than do the men; though as individuals they are also less constant.

(6) The constant error in horizontal lines is largely an individual matter. In the longer lines, however, there is a decided tendency to overestimate the right half. In the shorter lines this tendency is less noticeable on the part of the men, while in case of the women it is reversed, that is, the left half is overestimated. With vertical lines the constant error is very conspicuous even when the lines are short. It increases rapidly with the length of line. There is also less difference between men and women than in case of horizontal lines.

(7) Weber's law is probably valid for eye measurements of the middle of lines not longer than one meter. For longer lines few data are accessible.

(8) I think it has been shown that many of the conclusions of writers on this subject are based on insufficient evidence. This seems probable from the contradictions encountered among them, and from the personal differences described in this paper. While this conclusion is especially true of the various forms of constant error, it has greatly influenced the treatment of theoretical questions.

(9) Perhaps the most important result of this paper is to emphasize the value of the statistical method as supplementing the exhaustive investigation of a few individuals. Law certainly governs in mind as well as in matter, but the complexities of individual organization render its demonstration more difficult. The "normal" man is pure fiction.

(10) The regularity of results and the simplicity of method and required apparatus mark this exercise of estimating the middle of lines as worthy of an early place in all laboratory courses, and especially in schools without well-equipped laboratories.

AN EXPERIMENTAL STUDY OF MEMORY COLOR AND RELATED PHENOMENA[1]

By Grace Kinckle Adams

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I. Introduction

"The color in which we have most often seen an external object is indelibly impressed upon our memory"[2]; and, while this color may be aroused for various reasons, "it is awakened in a unique way if we see the external object again, or merely think we see it, for then it also determines what we see." This 'true' color of the object Hering calls the 'memory color.' "All things which are already known to us through experience, or whose color we think we know, we see through the spectacles of our memory color and therefore quite differently from the way in which we should see them without these,—provided always that we are not paying particular attention to the color." Katz,[3] while he adopts the term memory color, modifies Hering's definition. "Hering tacitly assumes that we see external things most frequently under normal illumination"[4]. Katz finds no grounds for this assumption, and, until it has been proved, prefers to define memory colors as "the surface colors in which familiar objects appear in normal illumination." He does not either agree that familiar objects always appear differently when seen through the spectacles of their memory color; for "if an object, for which one

[1] From the Psychological Laboratory of Cornell University.
[2] E. Hering, *Grundzüge der Lehre vom Lichtsinn*, 1905-20, 6ff.
[3] D. Katz, *Die Erscheinungsweisen der Farben*, 1911, 214 ff.
[4] *Ibid.*, 91 ff., Katz' discussion of 'normal illumination'.

has a memory color, is seen under normal illumination, the momentary color sensation should be identical with the memory color."

Hering noted the phenomenon and named it; he and Katz together have defined it. We wish to give this phenomenon a systematic setting. Hence we must, first, study the definitions of memory color offered by Hering and Katz, together with the examples furnished by them; and so, if possible, determine the conditions necessary for its occurrence.

The first point, upon which we find the definitions of the two investigators in agreement, is that the memory color attaches to a familiar object; and, from their methods of eliminating objectivity,[5] we may be certain that for them the significance of the term 'object' is the same. We may therefore assert with assurance that the primary and fundamental condition necessary for the appearance of the memory color is the object-consciousness. This first condition places the memory color in general perspective, and shows us that we are to seek it, not among the data of the observational attitude, but in the perceptions of our everyday, interpretative lives.

Of the identity of the two meanings of 'familiar' we can not be so sure. For Hering, the term seems to imply, as does his use of the word 'memory,' a recognition, or what we may call an individual identification, of a particular object. Katz nowhere challenges this implication; yet he follows Hering in giving examples in which the recognition is not of an individual object, but of a member of a familiar class of objects. Whether recognition, even in this broader sense, is necessary is a question which neither Hering nor Katz answers; we shall return to it later in this paper. About the familiarity itself, however, we must say one more thing, before proceeding. We naturally think of long acquaintance with an object as necessary to its becoming 'familiar'; but neither Hering nor Katz offers any reason to show why a single previous experience, provided it were sufficiently impressive, should not be adequate to the same result.

Two more claims, which Hering makes for the memory color, and which Katz, since he does not challenge them, apparently accepts, remain. The first is that the memory color is effective not only "when we see the external object again," but even "if we think we see it." Correctness of interpretation, in the commonsense significance, is plainly not a condition of the memory color. Several of Hering's instances bear out this fact. He offers none to exemplify the second claim, that we see objects through the spectacles of our memory color, only "provided

[5] See pp. 382 f.

we are not paying especial attention to the color,"—unless he means, by this "especial attention," not attention to the color at the expense of other characters of the perception, but an especial kind or mode of attending, which we might distinguish as the 'critical attitude.' There is support for this latter interpretation; and we may therefore tentatively accept a casual attitude toward the memory color itself as its third necessary condition.

In summary, we find the following to be the conditions essential for the appearance of a memory color; (1) the object-consciousness; (2) familiarity with the perceptive object, which —here we are anticipating the results of our own study— may amount to nothing more than the lack of any feeling of strangeness toward this object; (3) a casual attitude toward the memory color itself. It is clear that there is nothing inherent in these conditions which should of necessity limit the transforming image to visual quality alone; and a more careful consideration of them leads to the supposition that similar images may occur in any other sense modality. That they do occur in the case of tones and odors Stumpf[6] and Henning,[7] respectively, demonstrate. Moreover, Hering and Katz themselves give instances where there are transformations of texture and size as well as of color. For this generalized memory color, which may include any quality, as well as such perceptive characters of objects as form and texture, we suggest the term *imaginal overlay*. Our problem in regard to this overlay has been twofold: (1) we have studied the instances of it which we found occurring in everyday life; and (2), on the basis of these casual instances, we have devised experiments, whereby the overlay itself could be studied in more detail.

II. Casual Instances

In our study of these everyday instances, we found their *means of discovery* (D) to be, in the main, three.

D (1) An object is perceived; at first neither the interpretation of this perception nor any of its experienced aspects excites *O*'s scepticism; yet some extraneous happening makes him realize that the perception has changed, or is changing, and that there has been an overlay in the experience.

(T)[8] A patch of cement flooring near the shower bath was being resurfaced. As I went into the room I saw that the workmen had relaid the

[6] C. Stumpf, *IV. Kongress für Psychologie in Innsbruch*, 1910, 260.
[7] H. Henning, *Der Geruch*, 1916, 27 ff.
[8] The initial before every instance indicates the *O* who reported it: (T) Dr. E. B. Titchener, (Ho) Dr. L. B. Hoisington, (D) Dr. K. M. Dallenbach, (Bi) Dr. H. G. Bishop, members of the instructing staff, and (Br) Miss C. C. Braddock, (Fe) Mr. S. Feldman, (N) Mr. J. P. Nafe, (Re) Mr. A. C. Reid and (A) the writer, graduate students in the department of psychology. The unmarked instances were reported by undergraduates.

rug beside the shower bath in such a way that a triangular corner of it extended over the wet patch of new cement. This corner of the rug was soaked through with wet; the pattern is a mixture of dull brown and dull red; and I very clearly saw how the wet had quickened and livened the two colors. I stepped forward—it was only two steps—to pick up the rug and throw it back, and I saw at once that the triangular patch I had been observing did not belong to the rug at all, but was a shadow cast by the angle of the base of the shower bath upon the dry cement floor; the shadow, as I now saw it, was bluish grey.

This is one of the most absolute observations of the sort that I have made. The contrast between the wet and the dry colors in the pattern was as clear as daylight. In point of fact, however, the triangle could not possibly have belonged to the rug. If you think of the shower bath as a square, then the rug is a long rectangle lying just beneath the base of the square and barely flush at its two ends with the sides of the square; the shadow lay along the right hand vertical of the square, beginning from the lower right angle. One would have thought that this displacement should prevent such an error as I fell into; but the spatial impossibility was disregarded and, as I have said, the colors in the shadow were absolutely distinct.

I had no special set for this particular occurrence, but I have often been annoyed by the children's leaving the wet rug on the floor instead of throwing it over the horse. The sight of the wet area was therefore in general entirely familiar to me.

D (2) An object is perceived; and, although the perceptive experience is adequate to the meaning of the perception, the *O*, more or less casually, challenges the experience itself, or its interpretation. This challenging leads to the disappearance of an imaginal overlay.

(T) I was looking out of the window, watching for the street-car, and I saw through the shrubs by the fence the brilliant red slats of a familiar truck; just patches of red, brilliant scarlet. As I looked, it occurred to me that what I was really seeing were dead leaves on a tree; instantly the scarlet changed to a dull chocolate brown. I could actually 'see' the change, as one sees such changes in a theatre with a shift of lighting. The scarlet seemed positively to fall off the leaves, and to leave behind it the dead brown.—I tried to recover the red by imagining the truck, and found that I could redden the leaves somewhat; then I made them leaves again, and found that I could brown them somewhat; but I could not get either the original scarlet or the later dead chocolate.

I went out to see what the color 'really' was, and found it to be a distinctly reddish brown. Hence this observation has in it *two* colors (scarlet of truck and dull brown of leaves) which emerged, as if by way of emphasis of a partial factor, from the true reddish brown of the leaves seen close.

D (3) An object is perceived; but the inadequacy of the perceptive experience for the interpretation forces the *O* to a critical challenging of the perception; and a partially adequate overlay disappears.

(T) I was passing through the back hall and my eyes fell upon a woman's umbrella standing in the umbrella stand; the handle was of dark wood, almost unshaped, and was pierced by a large loop of heavy twisted cord. This I saw as the hilt of a sword. The silk broadened out into the braided grip, and a line of light along the wooden handle became the steel of the guard. The hilt was seen, so far as I can say, with perfect distinctness for the first moment.

I was slightly set to see a sword because yesterday evening I had missed one of the family swords from its usual place and had casually wondered whether my son had carried it upstairs in the holidays.

The curious thing about this illusion is that guard and grip changed places: that is to say, just as the silk cord stood out from the shaft of the umbrella, so apparently did the grip of the sword stand off from the scabbard; and just as the light line of the handle was continuous with the shaft, so the steel guard seemed to be continuous with the scabbard. I think it was this discrepancy which called my attention to the object more critically, when the sword hilt disappeared and the umbrella was seen plainly enough.

As these instances differ in ways other than those regarding the manner of their discovery, it should be evident that a mere detailing of our incidents, under these three rubrics, while demonstrating how frequent and how intricate is the rôle of the imaginal overlay in the common perceptions, would tell us little of the systematic significance of the phenomenon. Several cross-classifications are necessary to bring apparently heterogeneous cases into relation, and to disclose the range of the overlay. The first of these classifications concerns the condition of familiarity. Every object, in the cases cited, was familiar; yet the familiarity of the truck was more immediate than the familiarity of the sword: every object was transformed; yet the transformation of the truck had to fulfill the requirements of an identification, while that of the sword merely suited the conditions of a certain occasion. Because of these distinctions we made the following groups, which we may roughly head *kind of perception* (P).

P (1) Individual identification; a particular object is identified and the transformation corresponds with this identification.

(T) I went into the half-light of the kitchen-closet for a teapot. We have two on the same shelf, of about the same form and size; one light chocolate brown, the other deep metallic blue. I wanted the brown one. But I deliberately passed it over and reached for the blue one, which I positively saw as brownish; only in the light of the kitchen did I note my mistake.—I made sure later, under the same conditions of adaptation, that the brown and the blue were easily distinguishable in the dusk of the closet. —I cannot say that I saw the brown one as blue; but I somehow saw it wrongly; and I certainly saw the blue one as brownish.

P (2) Generic identification; the object is identified as the member of a class, and the overlay permits of a more general interpretation.

P showed me a cake of dark brown soap and asked: 'Doesn't it smell good?' I smelled it, and I think it smelled like soap, scented with an unfamiliar perfume. I then noted the brown color and asked if it were dog soap. When P replied 'Yes', the perfume smell went entirely out and there was a strong odor of creosote. A little later J came into the room and picking up the soap remarked: 'This is imported sandal-wood; doesn't it smell nice?' When I smelled it this time the creosote was entirely gone and the unfamiliar perfume was there again. I am not familiar with the smell of sandal-wood.

P (3) Occasional transformation; there is no true identification, but the transformation conforms to a temporary set.

(A) I was lying in bed this morning, and as I thrust my arm back over the head of my bed I felt something round and very warm. I knew there was nothing of this sort behind my bed and wondered why I had had the experience. Then I remembered that at college there had been a steam pipe behind my bed, and that I had often been annoyed by hitting it with my arm when I first woke up in the morning. As I remembered this, the roundness went out of the experience, and I felt the hard wood of the window frame, still warm. A few minutes later I wondered how the window frame could have been so warm, so early in the morning. I felt the wood again, and it was slightly warm, but not nearly as much so as before. This was merely a mild warmth, while before it had been almost uncomfortably hot.

P (4) General transformation; there is no particular set, but the transformed object conforms to its general surroundings.

(T) I was looking from the back of the house across my own to a neighbor's garden. I saw that he had built a low wall of light-colored concrete, with an arch in it, semicircularly topped. I had the immediate idea of 'sunken garden'—I knew that he had one, though for some time I hadn't visited it. Then I suddenly had another idea—'no water there though!' As the idea came, the light concrete changed at once to dark grey, and the whole perception changed. The arch was now a round-topped break in a line of leafless bushes, and what had been concrete was a shingled wall and roof seen beyond the bushes. The structure was wet, very dark grey; I could see here and there the lines of the shingles, and I could see amazingly plainly (I say this in retrospect) the line of slate-coloured metal that capped the roof. I thought at once 'garage'. Then, immediately, the structure's lowness and smallness ousted that idea, and 'dog-kennel' came. But just as this idea was forming, I thought 'memory color?' and looked more closely. Instantly the dark grey of the building changed to light grey, and the perception was realized as a stretch of uniformly dirty snow lying on the ground beyond the broken line of bushes, stretching across to a low bank the darker line of which had given the metal of the roof.

This light grey was positively darker than the light concrete, and positively lighter than the gray of the kennel. I am absolutely certain of these changes.

Having thus, by a classification, disposed of one condition of the overlay, we attempted to treat the remaining two, the object-consciousness and the casual attitude, in a similar way. On the basis of our data, however, we were unable to make the necessary classifications. Any change in the object-consciousness, which was not a direct and complete shift to the observational attitude, turned out to be nothing more than a voluntary or involuntary exchange of meanings within and during the object-consciousness itself. A classification in terms of anything which might be likened to 'levels of object-consciousness' had to be abandoned. For the casual attitude, however, there were occasional hints which seemed to indicate 'stages of criticalness'. Yet an examination of the reports failed to reveal any measure for this criticalness; the *O*s were, apparently, unable to say just how critical was the attitude in a given situation; all that they could usually report was that it had been voluntarily or involun-

tarily induced, and that it had completely or only partially ousted the overlay. A classification of criticalness of attitude was, consequently, also impossible. Since, however, the *Os* could often detect more than one meaning; since they usually knew if a meaning or an attitude was voluntary or involuntary; and since they could report on the transformations resulting from the various combinations of these factors, the following tentative classification was possible (M = meaning).

M (1) Only one meaning is realized; the attitude is quite casual and the perceptive experience is adequately transformed.

(T) I came through the back hall into the front hall and saw some strange man sitting in a chair waiting for me. My heavy overcoat had been thrown over a chair, and was in fact standing erect so that a triangle of black cloth appeared over the back of the chair. There was a small table in front of the chair on which lay a pile of newspapers, and a whitish corner of newspaper made the cuff and hand of the seated figure. The chair, which was back to me, stood at right angles to a couch, on the far end of which was a yellow cushion. This cushion, seen over the black cloth, formed the hair and profile face of the visitor.

As I caught sight of the figure, I definitely saw the hand move a trifle upward, and the whole body stir as if about to rise. These apparent movements evidently corresponded with a turn of my head made with the intent of bringing the figure into clearest vision. Then, all in a moment, as soon as I focused clearly, the figure resolved into its elements.

I do not think that I was set in any special way for seeing a visitor in the hall, although I have an insurance compact pending, and until things of that sort are cleaned up I am always a little uneasy about intrusive callers. I may therefore have taken this individual for the insurance agent, although all that I can positively remember is my irritation at finding that any visitor had been allowed to get access to me on a lecture morning.

M (2) There is a realization, more or less clear, of opposed meanings; the attitude is at first uncritical, but immediately becomes critical; the perceptive experience is partially transformed.

(T) In the shadow of a large box which stood below a window I saw a ten-cent piece on the floor. As I stooped to pick it up I distinctly saw the metallic lustre of the silver; at the same moment (so far as I can tell) I knew positively that the disc was *not* of metal but of paper. I was already moving, and went on to pick the thing up; it was a paper disc from the end of a spool of cotton, white, with a border of black; within this was a ring of print in thin capitals; across the white central disc enclosed by this ring were two lines of print in similar capitals. As it happened, I had my glasses off.

M (3) Only one meaning is realized; the attitude is to some degree critical; but the perceptive experience is still partially transformed.

(A) It was late in the afternoon; the electric lights were on, but colors out-of-doors were still plainly visible. I was looking out of the window and absent-mindedly watching a train travelling north. The train soon disappeared, but I continued to look at the smoke until it had almost vanished. Then my eyes wandered casually toward the south. At the extreme right of my field of vision, I saw a dark reddish purple road, which cut deeply into the earth. I think I realized at the time that there was something

bright and yellow about the color, but taking it as a road I continued to see it as purple. I also knew that I was too far away actually to see how deep it was. In spite of these two contradictions it looked like a road which absolutely belonged to the landscape; and I was willing to accept it as such. It was only after I realized that it ran diagonally across two fields, beginning abruptly in the middle of one and ending abruptly in the middle of the other, that I regarded it critically. Then the blue went out of the color and it became slightly orange (only very slightly), and I realized that the thing was much closer to me than I had supposed. It still meant 'road,' however, until I realized that it was the reflection, upon the window-pane, of an electric light in the room behind me. The color then became a very yellow orange and stood up in front of me.

M (4) A certain meaning is voluntarily put upon an experience; a casual attitude is voluntarily induced; the perceptive experience is adequately transformed.

(T) Thursday night. Coming down the gorge path, I took the bars of light across the ashes, near the top, for patches of glare ice. Only when I was actually avoiding them did I note the yellow-brown color and the uneven surface. A second lot of bars of light, lower down, failed utterly to give me the illusion.

Friday night. I looked for the upper patches of light, and got for a moment the appearance of ice,—only for a moment. The lower patches gave no hint of the illusion.

Saturday night. I walked blindfold down the path to near the lower patches; when I saw them, I had definitely, for a few seconds, the illusion of ice; but I could not hold it.—I did this in order to see if the lower patches would, under favorable circumstances, be able to produce the effect of ice.

M (5) A certain meaning is voluntarily put upon an experience; the attitude is almost casual; the perceptive experience is partially transformed.

(Bi) I was walking down State Street near the Infirmary when the sky was still light, but only about as light as the building in question, Rothschild's I think, when it was illuminated by the city lights. I cannot recall whether the sky was colored yet, or whether I had seen any colors this evening. I think, however, that it was, very probably, slightly colored at the time of my observation. My attention was attracted to what I took to be a drop in the horizon like a valley between the hills far across to the west. I saw a faint but clear red and above it, I think, a blue violet. I took the colors to be purely a perception of the sky. There were a few darker spots, very vague in outline but roughly rectangles with the long axis vertical. These spots caught my attention next. I think I changed both accommodation and convergence, and I saw then that the spots were windows. Practically simultaneously with the perception of the windows, the true yellowish white of the walls came out, and the color of my pseudo-sky had gone, as well as the sky itself. Then, just to see if I could, I let my original attitude return and the wall became colored again in a chroma about a third as good as that of the original.

M (6) There is a realization of one or more meanings; the attitude, whether voluntarily or involuntarily induced, is entirely critical; the perceptive experience is untransformed.

(A) Under an illumination from two oil lights, I took my new lavender hat from its box and showed it to a friend. She remarked "What a lovely pearl grey!" Until then the hat had seemed distinctly lavender to me; but as she said this I examined it critically and found the color to be a soft light grey, with no suggestion of color.

The previous references to partially adequate overlays warrant, as the facts themselves require, a final classification in terms of the *adequacy* of the overlay itself. The overlays, ranging from the completely adequate to the totally inadequate, fall into the following five categories (A).

A (1) Complete adequacy; the perceptive experience is so adequately transformed that the overlay can not be distinguished from untransformed sensory experience.

An historical instance of this is the following. Shortly after the discovery of potassium, Dr. George Pearson happened to enter the laboratory in the Royal Institution and upon being shown the substance, and interrogated as to its nature, he without the least hesitation exclaimed, 'Why it is metallic to be sure,' and then, balancing it on his finger, he added in the same tone of confidence, 'Bless me, how heavy it is!' "[9]

A (2) Partial adequacy in the form of a compromise; the characteristics of what would be an adequate overlay are not realized, neither are the characteristics of the untransformed experience.

(A) As I had my cup up to my mouth and was drinking, I heard someone ask: "May I have another cup of cocoa?" I immediately tasted cocoa. Then I suddenly remembered that I took coffee for breakfast regularly these mornings, and wondered how the waiter had made the mistake. I then became critical and finally found myself drinking coffee. There was an interval, however, between tasting the real cocoa and the real coffee, when the experience still meant cocoa, in which I could not have said from the bare 'taste' just what I was drinking. I got the taste of bitter and sweet, but the characteristic smell of neither of the two things.

A (3) Partial adequacy in the form of a combination; one necessary aspect of the perception is adequately transformed, another remains untransformed.

(T) As I was taking a cup of coffee from the maid's hand, I saw that the cream had been spilled from the pitcher over about a third of the tray. I said nothing, though I wondered why it had not been wiped up; and I was a little surprised to find that the cream pitcher was nearly full. I saw the smooth oily surface of the spilled cream distinctly; I noted also that the spill had run out here and there along the edge into blunt trickles. In spite of the twofold wonder I had no doubt at all of the reality of what I saw.

When I put my cup down on the tray, I saw that the pot was standing eccentrically upon an escalloped doily. This was my spilled cream and its trickling runners. The doily was of coarse white linen.

A (4) Partial adequacy in the form of a conflict; the conflict is between an overlay and the untransformed perceptive experience.

(A) From a box of assorted chocolates I took what I thought was a chocolate peppermint. When I bit into it I noticed that the color was light brown, yet I tasted it as fairly strong peppermint. A second later, however, while I was still tasting the peppermint, I began to taste caramel also. This conflict, backed up by the brown color, caused me to become critical. The peppermint flavor entirely disappeared.

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[9] J. A. Paris, *The Life of Sir Humphry Davy*, i, 1831, 268.

A (5) Inadequacy; a meaning is realized, but on account of the critical attitude the perceptive experience is not transformed.

(Ho) It was dusk, and as I was walking home my foot struck a common brown paper bag. I saw this color quite distinctly. I had kicked it along for a few feet, when I suddenly wondered if colors were really visible in that light. I then looked critically at the bag, and could not see a particle of color. It was merely a very dark grey.

These classifications are tentative, of course, but they should aid in the appreciation of the range and significance of the overlays in our everyday perceptions; the example under every rubric has been quoted in detail to illustrate the exact part played by the overlay in the particular perception. To make the illustration clearer and more general, we shall now exhibit, under rubrics representing their manner of discovery, other instances of the overlay which we have collected. Under every heading of this classification come first the examples of visual overlays, colors and perceptive characters, and then those of transformations in the other modalities. After every instance are symbols representing its place in the other classifications.

D (1) An object is perceived; at first neither the interpretation of this perception nor any of its experienced aspects excites *O*'s scepticism; yet some extraneous happening makes him realize that the perception has changed, or is changing, and that there has been an overlay in the experience.

a. The inspection of an object is interrupted, and when it is resumed the object is found to be changed.

(Re) I was walking down the street and I saw a big arc-light on the other side of the inlet. It had the orange color of an arc-light. I looked away for a while and then back at the light again. This time I realized that it was a fire, and I saw immediately that it was much redder than it had been when I took it for a light (P 4; M 1; A 1).

(Re) I was at dinner, and on the table in front of me were two silver candlesticks. I saw the polished silver quite plainly; it was very shiny and opaque, like the silver of the candlesticks we used to have at home. I looked away from the candlesticks to speak to my neighbor at the table, and when I again looked at them I saw that they were made of colorless, semi-transparent glass (P 2; M 1; A 1).

(Re) I heard the chimes play the first four bars of the Marseillaise, then an automobile came by and its noise drowned out the chimes for about ten seconds. When I heard the chimes again, a tune with which I was quite unfamiliar was being played (P 1; M 1; A 1).

(A) I was studying and heard the chimes playing 'Narcissus'. I do not know how long I heard this, but after I had been listening for a considerable time, my room-mate asked me a question. I did not hear the chimes at all while she spoke or while I was answering her. Just as I was settling back to my book, however, I noticed that 'America' was being played. America is played at this particular time every Sunday. I had recently had an argument with a friend about the similarity between 'Narcissus' and the 'Soldiers' Chorus' from *Faust*, and had, since that, listened for these two pieces to be played on the chimes, to see if I could discover wherein the similarity lay (P 1; M 1; A 1).

b. An interest in the perception causes *O* to examine it more intently.

(T) A tree had blown down in the back yard. I went out to look at it, and as I was going out of the door noticed a triangular chip out of one of the porch steps. I supposed that the falling tree had made the chip and went over and examined it. Upon examination it proved to be merely a triangular piece of sunshine lying on the step. Even when I saw this as a chip there was, I think, something pinkish about it, but because it meant chip I disregarded the pink and saw it as yellow. The sunlight itself was in point of fact pink (P 3; M 2; A 4).

(A) H often wears a medium blue sweater with a head-band which matches it exactly. She came into my room to-day and I saw the sweater and band, and they had just the same color. I was about to remark how becoming the color was, when I looked at the band rather critically and the blue went almost entirely out, leaving a grey with a slightly bluish tinge. I spoke to H about this, and she said that several other people had had the same experience, and that she thought there must be a good deal of blue in the grey. After a while she put on her coat, which was a pepper and salt mixture, and completely covered the sweater. I looked at the band then and could see no blue in it. I asked her to open her coat and let me see the sweater again. When she did this the band looked slightly blue again. The blue could be forced out by looking at the band very critically, but it persisted under more or less casual observation; it never, however, was nearly so blue as it had been the first time. The first time it had been just as colored as the sweater, but after this it was washed out, rather poorly saturated, blue. (P 1; M 1, M 3, M 6, M 5; A 1, A 2, A 5).

(A) Just before my eleven o'clock class I heard some one say that it was the fourteenth of March. I remembered that this was 'Class Day' at the college I had attended two years before, and I had a quick reference to the campus and the college buildings decorated in green, the class color. I thought no more about the fact, however, until the chimes began to ring at one o'clock. Then the idea of class day and the color green came again, but only for a second. Ten minutes later, as I was looking over my mail, I found a letter addressed to me in handwriting with which I seemed to be vaguely familiar, but which I knew belonged to no one with whom I corresponded regularly. When I opened the letter I saw that it was typewritten and began "Dear Class of 1920." I glanced rapidly over the first sheet and noted something about plans for a reunion, and then turned over to the second sheet to find whom it was from. At the bottom of this second sheet, I saw the signature of our alumnae secretary in the very same green of our class color. I thought "Isn't that typical of the girl, to use green ink to prove that she has a lot of class spirit? I believe the envelope was written in green too." I found the envelope again and, when I first looked at it, its writing had just the same color as the signature; but, as I continued to look at it, it seemed to become blue and then green again, so that I was really unable to tell its true color. To be certain I asked some one, and when she replied "O, most certainly blue," the green went out completely and staid out. I looked at the signature on the letter once more and it also was quite blue (P 3; M 1, M 2, M 6; A 1, A 4, A 5).

For several days I had been passing, at the same time every day, an iron fence about a hundred feet long, which had its preliminary coat of orange paint. On seeing the orange color, the fourth or fifth day, I muttered something to the effect that the color was a blot on the landscape, and almost simultaneously I realized that the fence had been painted black (P 1; M 1, M 6; A 1, A 5).

(B) On a couch across the room I saw a pillow cover with an Indian head on it. The face was seen in profile and there were lots of feathers sticking up above it. These feathers were red on a white background; the face itself was pink. I was interested in the pillow and walked toward the

couch to examine it more closely. As I got nearer I saw that it was a Cornell cushion. The pink face was the yellow and white seal, the white background the letters C O R N E L L, and the feathers the red of which the cushion was made (P 2; M 1, M 6; A 1, A 5).

(T) My coat was hanging upon a nail. Across one sleeve I saw a wet streak which seemed to have been made by leaning against a wet shelf. I could see that the colors had been livened and quickened by the water and that the streak was still wet. Wondering how it came there I looked at it more carefully, and found the streak to be only a shadow lying on the coat, and the colors in the shadow to be darker but duller than the rest of the coat (P 4; M 1, M 6; A 1, A 5).

(Re) I looked up from my book and saw a rose lying in the middle of my desk. In a more or less casual way I had been looking at it for several minutes, and I had seen the pink color and the soft texture of the petals quite distinctly. Then I suddenly wondered where it had come from and looked at it more carefully. Immediately the color began to change and become faded, and I realized that it was an artificial rose. Then quite gradually the texture began getting clothy. I could see it getting more and more like cloth; some parts became cloth sooner than others. Finally the whole thing was cloth (P 2; M 1, M 3, M 2; A 1, A 2, A 3).

(Bi) Up in the workshop Dr. D had been making a cast for observations to be made with the Titchener quadruple pendulum. I knew what had been going on, but had forgotten about the pendulum and the cast for the moment. I turned toward the window above the work-bench. The apparatus was on the bench about directly in front of the window. As my eyes passed to such a position that the plaster block fell just off the fovea, I saw the top of the block of plaster just like aluminum which is unpolished but still smooth and bright. I think the sides of the block which were under poorer illumination did not become metallic. I am sure I had the meaning of aluminum, and there was a half-formed query which completed would have been, "why, how did he get the aluminum?" The perception vanished under scrutiny, but while it persisted it had the sheen and fine grey flecks of metallic lustre. It did not return; but I tried to give it a chance to return. I think the plaster stood out, after the metallic look had gone, as it should have, very white (P 4; M 1, M 6; A 1 or A 3, A 5).

(T) As I walked forward through the hall toward the front door, I saw lying on the floor to the right, as if the parcels-post man had just thrust it in, a large and rather battered looking package. It was evidently a square cardboard box which had suffered in the mail; it was wrapped in heavy brown paper, and I clearly saw some of the folds and crumpled edges of the wrapping as well as the lines of the string with which it had been tied. I can't positively say if I saw a cross of string with a knot in the middle of the cross, but that is definitely my impression; anyhow I saw lines of string very plainly. I swerved to the right to pick the thing up and found that it was simply the end of a rug projecting into the hall from the drawing-room. The rug was a particolored oriental affair, with a silky nap, and the light falling on it from the drawing-room windows had turned it into a sort of general pinkishness,—not at all the color of the lightish brown wrapping paper, but still near enough to that to show me how the illusion could have arisen. I tried to reproduce the illusory experience, by going back into the hall again, but could get only partial hints of it,—nothing at all as positive and definite as the original perception.

I was expecting an express package rather eagerly, though I had at the time no particular expectation of a parcels-post package. The set for the express may, however, have been enough to touch off the illusion (P 3; M 1, M 5, M 6; A 1, A 2, A 3, A 5).

I walked into my study and saw a small animal with brown fur rolling across the floor. I wondered how in the world it could have got there and

went over and examined it to see what kind of an animal it was. I found it was no animal at all but my metal trash basket. The trash basket is yellow, but its color was not visible in the darkness of the room (P 2 or 3; M 1, M 6; A 1, A 5).

(Bi) I was sitting in Prof. T's Goldwin Smith office facing the window with F off to my right, well off at the edge of the visual field. I happened to move facing him just enough to bring him into view vaguely. I think that the light tint of his sock caught my attention first. As the perception formed, and it formed very swiftly, the sock became silken with the fine pattern of light and dark grey flecks. It had, as my eye turned more squarely toward it, all the usual character of a silk stocking. There was a definite meaning of stocking, I think, due to the length of sock exposed; about two thirds of the distance to his knee. Upon examination this sock became the usual dull cotton garment with no sheen upon it and no transparency at all. I turned my eyes away again as nearly as I could to the old position, but the silky perception did not return (P 3; M 1, M 6; A 1, A 5).

(Bi) I was working in the shop and happened to glance in the direction of the sink, and I noticed that the soap in the rack above the sink was wet. I saw the slick moist surface of the soap quite plainly. I was curious, as I knew that no one had used the soap that hour, so I looked at it more carefully. As I looked the wet stripped right off and the soap became dry. I was able, however, by imagining first wet and then dry soap, to make the slickness come and go (P 3; M 1, M 4, M 6; A 1, A 5).

(T) A dust-cloth was lying in the back hall, where there is a prismatic effect that I have often noticed and even produced by laying objects there. As I passed through, I saw the cloth as a towel of unbleached linen with a variegated border. I saw distinctly that the fabric of the border was of different material from that of the body of the towel. As the thing was unfamiliar to me, I looked again, and now saw the grey dust-cloth with the light on it. It lay in folds and the light spread across the upper fold (P 4; M 1, M 6; A 1, A 5).

(T) A curled leaf on the corner of my dressing-table was taken for a flat splinter of mohogany chipped from some bit of furniture. I am not unaccustomed to finding such splinters; the leaf was a novelty. The color was approximately right; but I saw the lustre of the polished wood, whereas the leaf was rough-surfaced (P 4; M 1; A 1).

I was walking up the street and saw P coming toward me on crutches. I saw her very plainly, and noted her slim figure and athletic build. I was surprised, however, to see her on crutches, for I didn't know she had had an accident. For this reason I continued to look at her, and I suddenly realized that it was not P. Immediately she became much fatter and her whole physique changed. I was amazed that I could have ever thought she was P.—Several days later I was walking along the street and had exactly the same experience (P 1; M 1, M 6; A 1, A 5).

(A) I was dressing on a Saturday morning and I heard the chimes playing one of the familiar foot-ball songs. I knew there was to be a game that afternoon, but I wondered why that particular song was being played in the morning rather that at noon as it usually was. This caused me to listen more carefully, and I discovered to my surprise that the piece was not "From Rocky Heights" but "Marching through Georgia." The rhythm and the grouping of the notes in the two pieces are similar (P 3; M 1, M 6; A 1, A 5).

(A) L plays the victrola every morning. To-day I heard it playing in the next room. I could hear the syncopated rhythm and could hear the tones go up and down, but I could not recognize the tune. I listened intently for several seconds before I realized that it was not the victrola but the carpet sweeper in the next room. The notes of the victrola were more distinct and I think higher than the buzz of the sweeper (P 3; M 1, M 3, M 6; A 1, A 4, A 5).

c. The *O* acts upon the object; and the object changes.

(A) Two months ago I bought some purple handkerchiefs to give to my *O*s. I had been very careful about getting a certain quality of purple, and had, on that account, become very familiar with that particular color. To-night I saw one of these handkerchiefs lying on my dressing-table. It was soiled and I saw the purple color most distinctly. I wondered how it got there; but I never for a moment doubted the reality of the color; although, if I had thought, I might have known that the purple wouldn't be so good under the yellow light. Still, when I saw that it was soiled, I reached for it to put it in my handkerchief bag. Just as my hand reached the handkerchief, the purple faded completely out and left behind a decidedly yellowish brown. The handkerchief was a yellowish brown one that had been given me a few weeks before and which I had not used very much (P 2; M 1, M 6; A 1, A 5).

(T) I had my 'near' glasses on when I came into the room. I had been wrapping up Christmas packages and I saw the end of a piece of twine lying on the desk. I saw the separate strands forming the twine, the shiny surface of the string, and the cut end. The details were absolutely complete. When I picked it up, it was a dull gray elastic band (P 3; M1, M 6; A 1, A 5).

(N) A little while before I had found a dime on the floor of the lecture room. When I walked into my sitting-room a little while ago I saw another dime. It lay on the floor just opposite the window. I went over to pick it up, and found that it was only a mother-of-pearl button with two holes in it. I went back to where I had first seen it, and by persuading myself that the button was a dime could make it look like a dime again. I could see the silvery lustre, and the two holes in the button became the face of the coin (P 3; M 1, M 4; A 1).

(A) My hands were damp, and I reached for the towel I saw lying over the back of a chair, to dry them. The towel was a plain white Turkish one with a coarse weave. It lay crumpled up so that I didn't see its shape. My hand had almost reached it, when the coarse weave turned to wooliness and tiny pink stripes came into the white. I realized that I was picking up the coat to my flannelette pajamas. (P 2; M 1, M 6; A 1, A 5).

(Br) As I was coming down the stairs I saw a lot of white palings leaning against the wall opposite the railing. They stood out from the wall, they were a very good white, and the edges were sharply defined. I put my hand out to touch them and as I did so I realized that the edges were not sharply defined. Then the meaning of 'shadow' came, and I saw that the shadow of the railing itself had given the effect of palings. Immediately what had seemed to stand out from the wall became flat and the white became yellowish (P 4; M 1, M 2, M 6; A 1, A 3, A 5).

(Br) I was looking for letters in the post-box and I saw a brown paper package in the box. I could see the box standing out from the wall and there was a light wooly string around it. As I reached for it I realized that there was really no package there. Immediately the brown thing went back against the wall, and I saw the grains in the wood of which the mail-box was made. The string got darker and darker until it became a black shadow (P 3; M 1, M 6; A 1, A 5).

(A) I was coming home late in the afternoon; it was almost dark and the light on the front porch had been lit. I turned in at the gate and walked toward the front steps. As I reached the bottom step I saw a box wrapped in white paper lying just at the front door, partly in the light, partly in the shadow. I saw the whole of one of the long sides of this box,—the top of it receding back from me into the shade,—and a tiny bit of one of the smaller sides. The paper was very white and I saw its smooth texture, but I cannot definitely say that I saw any string. I would have picked the thing

up, but when I reached the top step I found that there was no box there, but merely the rounded base of one of the posts which stood on either side of the door. This was made of wood and was a fairly dark grey.

I was not expecting a package; but when at home I am not unaccustomed to finding packages by the door at this time in the afternoon. I think the time of day was probably enough to touch off the illusion (P 3; M 1; A 1).

(A) I was doing my hair before the mirror and I saw a streak of powder on my chin. It was white powder and much lighter than my skin. The rough texture of it stood out noticeably. I started to rub it off, but I found that it wasn't powder but a small beam of light lying on my face. It lost its rough character immediately and became nearer the real color of my face; but I was able to see this small portion of my chin as different from the rest, as if a small piece of another skin, whiter and finer in texture, had been grafted perfectly into my face.

The critical attitude evidently prevented a second overlay by which the texture and color of this portion would have seemed the same as the rest of my chin (P 3; M 1; A 1; P 2; M 6; A 5).

I was knitting a sweater on half of a hair-pin off which some of the enamel had been rubbed, so that the glint of the metal could be seen. I dropped the hair-pin on the rug and stooped to pick it up. I saw it plainly; the glint of the metal stood out against the dark of the rug; but to my surprise what I tried to pick up was not the hair-pin but a thread in the rug itself. This thread was light in tint but was composed of several different colors (P 1; M 1; A 1).

(Re) While working in the laboratory yesterday morning I got some calcium on my clothes, and all day I was dusting little flecks of it off my coat. Late this afternoon I saw one of these white spots. I didn't doubt it for an instant, but started to flick it off. Immediately—almost in a flash—the color changed to yellow and I saw that I was looking at the sun shining through a button-hole (P 2; M 1, M 6; A 1, A 5).

(A) I reached for the sugar bowl and started to take out a lump of sugar. The particular lump I wanted looked just like all the rest; the reflection from the gold lining of the bowl made them all look slightly yellow, but this one had the same rough texture as the others and I saw it stand out from the sides of the bowl. Just as I was about to touch it, my chosen lump disappeared and I found myself trying to pluck out a piece from the side of the bowl itself. The shape was given this pseudo-lump by the position of the real lumps (P 2; M 1; A 1).

(Re) I was manipulating the motor in an experiment. When not actually working I had been absently sticking a nail into some old nail holes in the table upon which the motor stood. The holes were round and even and of course darker than the yellow wood of the rest of the table. I watched the nail as it went into each hole, but suddenly it reached a hole where it could not go in. I then looked at this place more carefully and it was nothing but an ink blot. Instantly the dull dark of the hole changed to the shine of the ink and the regular outline became uneven (P 2; M 1, M 6; A 1, A 5).

I saw a drop of water on the table-cloth and showed it to the persons on both sides of me, telling them to watch it crawl up my place-card. I put the card under the drop and the drop began to move. Then I pulled the card away from the table and I could feel the strain in my arm as I drew it up. I did this several times and all the while I saw the glistening surface of the water. When, however I put my hand down on the table, it was dry, and I found that my drop had been merely a reflection from a drop of water clinging to the inside of my glass. It was simply a darker spot on the table-cloth and had nothing watery about it. By putting the card on

top of the shadow I could make the shadow appear to climb up the card, but it did not look watery again; neither could I get the pull in my arm (P 2; M 1, M 6, M 5; A 1, A 3, A 5).

D (2) An object is perceived; and although the perceptive experience is adequate to the meaning of the perception, the *O*, more or less casually, challenges the experience itself, or its interpretation. This challenging leads to the disappearance of an imaginal overlay.

a. There is a disagreement among two or more people about the perceived object.

(T) I saw a strange pocket-handkerchief, with broad band and spots of brown, lying on the table under the lamp. I said to my wife: That's not ours—one of the girls must have left it. She said: Why, that's my lavender handkerchief! I asked her if she really saw it as lavender, and she said: Certainly,—come here and see the light showing through; you'll see it's lavender. I still saw it as brown; and my wife said presently that it now looked brown or brownish to her. I could not get her to go behind her 'knowledge' that it was lavender; but so far as I could tell in her critical seeing of it it was brown (P 1; M 1, M 6; A 1, A 5).

(D) We were having what I thought was 'riced' potatoes for dinner. I looked at the creamy yellow color of the potatoes and said to myself, 'I wonder why my wife didn't have rice!' As I don't usually eat potatoes I did not take any, and as my wife saw me refuse she asked, 'Aren't you going to have any rice?' As she said this the yellow of the potatoes faded into the white of the rice. I tried to get the yellow back again by imagining the potatoes, but was unable to get any change in the color (P 4; M 1, M 6; A 1, A 5).

(A) Until I was eleven years old my hair was of a medium shade of brown. When I was eleven, however, I had an illness and lost most of my hair. The new hair which came in was quite dark. I noticed this and my friends and family often remarked about the change. Still even now—ten years later—when I see my hair in the mirror casually I see it as light, and am startled when any one likens it to hair which I see as much darker. When I look at it critically, however, it is always dark (P 1; M 1, M 6; A 1; A 5).[10]

(A) At the place where I was visiting the family was much excited because Aunt F—aged seventy-five—had got a very youthful lavender dress. The dress, which was made of a lavender and white checked material, was shown to E and me as soon as we arrived, and then put away again. Several nights later, E and I were in bed and the door of the wardrobe was open so that we saw part of the skirt of a dress sticking out of the wardrobe. E asked C—whom E and I were visiting—whose was the brown dress. C answered 'Why that isn't brown, its Cousin F's lavender dress!' I then looked at the dress, and finding it to be of a very good brown informed C of the fact. She insisted that E and I were both crazy and that she still saw it as lavender. She opened the door so that we could see the whole dress, with the light on it in the same way as it had been shining on the exposed part before. As soon as she did this I too saw the dress in the same lavender in which I had seen it in the daylight. I could, however, by becoming critical, force the lavender out and the brown in, and again, by thinking 'that is Aunt F's lavender dress' make some of the lavender return (P 1; M 1, M 5, M 6; A 1, A 2, A 6).

(A) At the supper table I was using a handkerchief which had originally had a lavender border, but which was now so faded that it appeared quite grey under the lamp light. I asked D what color it was and he re-

[10]T and Professor H. P. Weld report that, when looking casually into a mirror, they see their hair, which is grey, as light brown and dark brown respectively. Under like conditions another *O*, who has been bald for six years, sees his head thickly covered with hair.

plied "Grey, of course." L, overhearing him say this, interrupted with "Of course it isn't grey. I have one just like it and it's lavender." I asked her if she really saw it as lavender and she assured me that she did, and was unable to see it any other way (P 1; M 6; A 5; P 2; M 1; A 1).

(A) It was 'Spring Day' and about time for the finish of the races. A friend was reading aloud to me when suddenly I heard a loud shout. I interrupted her to remark that we had probably won the race. She wanted to know why and I asked her if she didn't hear the cheering. "What cheering? All I hear is the water running in that pipe over there." The sound then changed for me, it became harder and more metallic and also more intermittent. As I listened, I could by thinking first of the cheering and then of the pipe make the sound change. It did not, however, get quite so much like a cheer again (P 3; M 1, M 5, M 6; A 1, A 2, A 5).

(N) I was coming up the hill with a friend, and as we approached Cascadilla, where we had intended to eat breakfast, we commented on being too late for their breakfast hour by a wide margin. As we came up beside the dining-room I smelled coffee very strongly and said "We are just in time for the coffee grounds." My friend laughed and replied, "You smell burned gasoline from that truck"—a truck pulling up to the side of the building. I couldn't get the smell of coffee again but am not sure that I ever smelled the gasoline, as a fresh wind came up just as he spoke (P 3; M 1; A 1).

(A) One of the girls in the house has an alligator. The smell from some meat which had been left in his pan had been very objectionable when I left the house this morning. When I opened the front door at noon, I smelled the odor strongly and wondered why it had not been attended to. I paid little attention to it, however, as I was rather intent upon getting my mail. The letter I was looking for was there and I was reading it while walking upstairs. When I got to the top of the stairs, I saw one of the girls and asked her why no one had cleaned the alligator's pan. "Why, the alligator's gone," she replied: "we moved him to the cellar." To be certain, I went into the room where the alligator had been and found no trace of the odor. To check the thing up I went back down stairs. As soon as I got about half way down the steps, I smelled a fairly strong odor of scorched food. Not only was the putrid quality gone from the experience but the odor was now more voluminous and less oppressive than it had been (P 3; M 1; A 1).

(N) In the morning we had been making plans for Thanksgiving and among other things we mentioned a turkey. That evening we went out for dinner. About an hour after dinner we were visiting some friends and M began to tell about the delicious turkey dinner she had just eaten. When asked if the fowl wasn't duck, she said no, she was certain that it was turkey. She was positive that she had tasted the turkey flavor and was almost as sure that she had seen the white meat. She would not be convinced that it was not turkey until her host assured her that it was really duck (P 3; M 1; A 1).

(A) This noon we had parsnips for dinner. They were sliced and fried in long thin pieces and had about the same color as a yellow sweet potato. W thought they were sweet potatoes and tasted them as such. She ate several and would not be convinced of her mistake until she asked the steward. After this she reported that they tasted differently and that she did not like them. She seldom eats parsnips because she dislikes them, but she is very fond of sweet potatoes (P 2; M 1, M 6; A 1, A 5).

I was eating cake with chocolate sauce. I had already swallowed one mouthful when some one said something about blackberry sauce. Immediately what I had in my mouth tasted like blackberry. I looked down at my plate and what had been dark brown a little while before was now dark

purplish red. I can't say that I positively tasted chocolate in that first mouthful, but I am certain that I tasted nothing like the later blackberry. We have cake with chocolate sauce about once a week and the blackberry was an innovation (P 2, M 1, M 6; A 1, A 5).

As I was passing through the kitchen I noticed a plate of rather poorly hardened fudge on the table. I went over and scooped a little up on my finger and stuck it in my mouth. It tasted like fudge which had too much salt in it. Because I didn't like the salt I didn't eat any more, but went on out of the kitchen. At breakfast the next morning my hostess asked me to have some anchovy paste and I recognized the dish of fudge. The salt was still there in the paste but there was a decidedly fishy flavor and no suggestion of chocolate. There had been no hint of fish in the taste of the fudge, although the taste of salt had been the most prominent thing in the experience (P 2; M 1; A 1).

b. The *O*, of his own accord, doubts a perception.

(T) Curiously enough, I was again tricked by the blue teapot only six days after my first experience. I went into the closet for the brown teapot, saw it on the shelf, and began to reach up for it. I had hardly begun to move when the memory of the former mistake flashed across my mind, and I clearly saw blue. There was no noticeable disjunction of time between the two looks,—I was still looking when the memory 'flashed,' and I kept on looking; the flash was momentary. All the same I saw brown before I saw blue. The brown teapot is a family possession of long standing, and I am therefore very familiar with it (P 1; M 1, M 6; A 1, A 5).

(A) Slightly less than a month after my first experience with the green ink, I found another letter from the alumnae secretary in my mail. The green of the address, as far as I can say, was precisely as it had been the first time. I cannot say that I first recognized the writing and then saw the green; the two things seemed to come at the same time. I merely saw a letter from the secretary addressed in green ink. I immediately became sceptical, remembering the earlier experience; so examined the color critically. It changed at once to a blue, which was darker than the green, and also darker than the blue of the former letter, with which I later compared it. Again I was able to get the green to return somewhat by thinking of the other experience and keeping my attitude fairly casual (P 3; M 1, M 5, M 6; A 1, A 2, A 5).

(A) For several weeks there had been a bluish green cloth on a screen in my room. Yesterday I removed the cloth but left the neutral grey screen in the same place. To-day when I came into the room I saw the blue on the screen. In a flash I remembered that the blue cloth was gone. The blue color was gone also. I am certain that the chroma of the blue to-day was poorer than it had been before, but that I neglected this fact until I realized that I was looking at grey cardboard and not blue cloth. I cannot say exactly what was the texture of this blue, but I have a definite impression that it was clothy. A few hours later the experience was repeated. And then I found that I could get some of the original blue back by 'imagining' the cloth hard enough (P 1; M 2, M 6, M 5; A 2, A 5).

(A) For three consecutive evenings I have had this happen. I have come home from the campus when it was too late for colors to be seen, but when the outlines of familiar objects were perceptible. As I turn a certain corner, I recognize a grove of pine trees, and I see the green of these trees. It is a fairly good green, but darker than that which these same trees have when I see them in the morning. Then I realize how dark it is and become critical toward these trees, and I find them to be a very dark grey with no trace or hint of green (P 1; M 1, M 6; A 2, A 5).

(A) I looked from the laboratory window and saw a great pile of dirt beside an unfinished building. I noted the deep red color of the earth, and

had been looking at it for several minutes when I realized that earth of that color was not common in this part of the country but was what I had been used to as a child. I then became critical, and I saw the dirt as a rather poor violet with very little of the original red. I repeated this observation several times with a more or less critical attitude (I say 'more or less' because I was unable to keep it entirely casual as I wished). At first I was able to recapture a little of the red, but finally I was unable to get any of it (P 2; M 1, M 6, M 5; A 1, A 5, A 2).

(Bi) I started to turn on the electric light but decided that I didn't need it. Still as my hand reached the bulb I saw the whole side of the room at which I was looking as if it were flooded with light (P 3; M 1; A 1).

(T) I looked out of the front window on getting up, and saw the pavement as dry; I was curious as there was to be a football game. It was not raining at the moment, but it had been raining heavily, and everything in fact was wet; but I overlooked the wet road. I then looked out of a side window, and saw that the roof of the porch was very wet. So I looked again out of the front window and saw the pavement as wet—and as very much darker than I had seen it the first time (P 2; M 1, M 6; A 1, A 5).

(Bi) Yesterday I cut my finger and it bled considerably. This morning there was a small dark reddish brown scab where the cut had been. Once when I looked down at this scab I saw it as red and shiny blood. As I continued to look at it, it suddenly changed to the dull, dark brown scab. As I looked it continually changed from scab to blood and back again (P 3; M 1, M 6, M 4; A 1, A 5, A 1).

(Bi) I looked out of the window of Morrill to see if I saw any signs of the snow thawing, and was surprised to see parts of the walk beside the building as wet and parts of it as quite dry. When I had looked at this walk a little while before it had been entirely covered with snow, and I did not believe that all the snow could have melted in such a little while. So I looked carefully and the wet and dry spots of the walk became dirty and clean patches of snow (P 3; M 1, M 6; A 1, A 5).

(Re) When I took my seat at the concert last night the conductor had not appeared and only a few members of the orchestra had taken their seats upon the stage. I could see a mellow yellow brown showing through the backs of the empty chairs. Just then the conductor came forward and I wondered "How is it that he is coming out before all of his men?" As I was asking myself this, the mellow brown as seen through the chairs turned to a light cream, and I realized that what I had taken for empty folding chairs were music racks with sheets of white music upon them. Not only did the color change but what had seemed filmy acquired the slick texture of creamy paper (P 3; M 1, M 2, M 6; A 1, A 3, A 5).

(Bi) I was at the seminary at Dr. T's, and at some moment of lapse of attention from the subject of the discussion I heard a complex sound like a full harmony of an orchestra or band. This perception ran a short course unchanged in quality. When I became critical, the complexity was rapidly reduced but the experience was still a chord of several notes. Very soon, because the intensity increased rapidly, I discovered that what I was hearing was a trolley car. Even after I knew it was a car, the tonal character of the sound continued until the car was opposite the house, when at this full intensity all harmony passed and I was left with only a rumble and the slight singing quality of the motor. The seminary was being held in the room in which Dr. T's orchestra plays on Sunday evenings (P 3; M 1, M 3, M 6; A 1, A 2, A 5).

(A) I usually come home to lunch while the chimes are playing. When I reach the gorge path the sound of the water coming over the falls is so strong that I can hear above it only the accented notes of the piece being played on the chimes. If a new melody is started while I am at this place I cannot of course recognize it. To-day I came home from the laboratory at

the usual time but as it was Sunday there were no chimes. Still, when I reached the place where the path is nearest the water-fall, I heard the accented notes of a tune written in two-four rhythm. I did not recognize the melody but I was sure of a change in pitch. Suddenly I remembered that it was Sunday and listened more closely. I heard only the rush of the water (P 3; M 1, M 6; A 1, A 5).

(Bi) I was walking down the corridor and I saw ten feet ahead of me a woman's glove hanging on the bulletin board. As I saw it, there was a positive odor of kid leather and a faint perfume—a definitely flowery odor. There was just a flash of these odors; for, as I wondered how I could smell the glove so far off, they both disappeared. I went up and sniffed at the glove; the scent of the leather was similar to what I had smelled before, but the perfume was quite different. The odors smelled close were no stronger than those that I had smelled ten feet away (P 2; M 1, M 6; A 1, A 5).

From a box of chocolates which my friend offered me, I took a large plain cream. When I had eaten about half of this piece, I decided I would rather have a peppermint if I could find one. I did find several peppermints and ate them. About five minutes later I saw the half eaten cream lying on the table and put it in my mouth. It tasted precisely as the peppermints had done. It wasn't until I had swallowed it that I realized that it was the other half of the plain cream (P 2; M 1; A 1).

(A) When taking a cold bath on an early winter morning, I notice that the room feels much warmer, if I turn on the electric light before getting into the tub. To make sure that the light did not actually raise the temperature of the room, for one week I measured the temperature before and after my bath with the light off, and for one week with the light on. There was no difference in temperature under the two conditions, but the difference in the feel of my body was amazing. When the light was off, my body felt chilled and I dreaded getting into the cold water; when the light was on my body felt quite comfortable and I anticipated with pleasure the cold plunge (P 3; M 1; A 1).

For many years I had to wear a brace on my foot. When the brace became dry it squeaked and I oiled it. To-day I was walking along with a girl who was dragging along an umbrella, so that it made a squeaky noise. As soon as I heard this, I thought—I must oil my brace. Then I felt very clearly the stiffness in my leg (P 3; M 1; A 1).

c. Another meaning for the experience comes.

I was sitting in a darkened room. A boy was standing in the lighted doorway so that I saw his figure in silhouette. I recognized him as Ba and I saw the yellow of Ba's hair. Then somehow I decided I had made a mistake, and that it was Bl that I was seeing. I then saw the yellow fade very quickly into black. But just as it had faded I thought 'perhaps it is Ba after all', and the yellow came back again. It was indeed Ba, but when I looked carefully I found that the yellow of his hair was not visible because of the light being behind him (P 1; M 1, M 6; A 1, A 5).

(T) On a very cold afternoon I went out on the kitchen porch to see if the door of the storm-house was firmly closed. The door faced directly the setting sun. As I pulled the door toward me, I pressed down the trigger of the latch, and so opened the slot in the door within which the trigger works. A beam of red sunlight flashed through the slot and crossed my thumb. I snatched my hand away, thinking I was to be burned by an electric spark; I had at the moment a swift reference to the children's scuffling on a wool rug, and then touching off 'snaps' from one another! Almost instantly I realized that the light was sunlight; but it was so brilliant, in the half obscurity of the storm-house, that I could almost if not quite recapture the illusion of the spark as I played with the latch (P 2; M 1, M 6, M 5; A 1, A 5, A 2).

(Re) I was walking down the street at night and I saw rough textured white frost by the side of the walk. As it was a cold night I did not doubt the frost, but suddenly I had the idea that I was looking at the large leaves of some lily plants which grew beside this walk. Then as I looked more closely the rough texture of the frost became slick and the leaves, although I could see no actual color in them, were darker in tint than the frost had been (P 3; C 1; A 1).

(A) I had my eyes shut while observing in an experiment in audition. When I opened them I saw a silvery lustre. At first this meant nothing to me and I asked myself 'What is it?' I recognized my black tie. The silvery lustre disappeared and I saw the ribbon as black with sunshine on it. By becoming casual I was able to see the black as grey again, but never as light or as filmy as I had seen it before I had the meaning of 'my tie'(P 1; M 1, M 3; A 1, A 2).

(Br) I heard the chimes play six notes of "Auld Lang Syne." Then I realized that an unfamiliar tune was being played. This tune was twice as slow as "Auld Lang Syne", so I had filled in three of the notes and heard the other three differently. The imagined notes were, I think, though I am not at all certain, duller and more thud-like than the others (P 1; M 2, M 6; A 2, A 5).

(A) Last night as I went from Morrill to White I saw Sibley ablaze with light. I knew that there was a lecture there. To-night I was walking from White to Morrill, facing Boardman, and when I got as close to Boardman as I had been to Sibley on the previous evening, I saw lights in all the windows of Boardman. As well as I can say in retrospect, at the moment I saw the lights I also heard the sound of applause coming from Boardman, and I thought, "The lawyers must be having a lecture!" I heard distinctly the intermittent sounds of clapping hands. Just then my attention was attracted by a large light beside Boardman and I saw that it was the headlight of a large truck or automobile. Then I knew that the sounds I had heard did not come from the building but from the truck. By this time the truck was at the top of the hill which it had been climbing and so the sound had possibly changed, but I am almost sure that the applauses were higher in pitch, lighter and more intermittent than any rumble of the truck could have been. They were certainly quite different from the sound of the moving truck as I now heard it (P 3; M 1; A 1).

(Fe) As I was passing by Cascadilla gorge I smelled a skunk. I glanced around, looking for the skunk, and I noticed an automobile ahead, going down the road. Immediately the odor changed to one of gasoline. I stopped for a while until the automobile was out of sight, and tried by sniffing to make sure that there was no skunk around. The gasoline odor soon disappeared, but the odor of skunk did not come back. Skunks are quite common around this gorge (P 3; M 1, M 6; A 1, A 5).

There were two boxes of wafers in a package my mother sent me. We opened one of the boxes last night and ate all of its wafers. They had a peppermint filling which was fairly strong. This morning I opened the second box and ate a few of the wafers. They had the same peppermint flavor as those of the night before. For some reason, however, I happened to look at the filling of my fourth wafer and found it to be brown, instead of white, as it should have been to be peppermint. There was no trace of peppermint in the wafers which I ate after this (P 2; M 1, M 6; A 1, A 5).

(A) I notice that at night when I first put my foot down in the bed I often feel the wooliness of the blanket. Then I ask myself how can it be the blanket as I am sure the bed is well made. Then I feel the harder, cooler, slicker, cotton sheet. Then if I wonder if perhaps the sheet has not come untucked and my foot is actually touching the blanket, I feel the

warm wooliness again. Often I have to feel down in the bed with my hand before I can persuade myself that the sheet is well tucked in (P 2; M 1, M 4; A 1, A 1).

D (3) An object is perceived; but the inadequacy of the perceptive experience for the interpretation forces the *O* to a critical challenging of the perception; a partially adequate overlay disappears.

(T) I was looking from the back of the house across to the Knoll, and saw a small car going up the sloping road; it was a small closed car like an electric brougham. Behind it at a short distance came another odd-shaped car with some erection upon it; I took it for the moment to be one of the students' cut-down Fords. I thought nothing of the observation and was looking away when it struck me that the two cars were moving at an absolutely even rate, almost as if they were mechanically joined. I then saw that the two of them together were in fact one immense truck. The driver's house had given me my electric brougham; then came an immense stretch of yellow canvas which I had simply identified with the grey sky behind; and my Ford with something standing up on it was the dark oval at the back of the truck, which was not closed up, so that I could see a little distance through the thing under the hood of canvas. The decided way in which the two original cars snapped together when once I realized that they belonged to the same truck was excessively comic; I found myself laughing aloud though there was nobody present to whom I could explain the joke. The sky was sheer grey, the grey of fairly uniform rain-clouds; the canvas of the truck was a fairly chromatic light yellow (P 4; M 2, M 6; A 3, A 5).

(A) I was walking down the street and saw the smoke coming out of the chimney of one of the near-by houses. There was a lot of the smoke and it had the characteristic bluish tinge of the smoke from a fire burning well, out-of-doors on a clear day. As I walked on, I noticed that the smoke had no movement at all. So I looked more intently and realized that it wasn't smoke at all, but the snow on a steep bank behind the house. The trees near the house had given the smoke its proper shape, but most of the blue faded out when I realized that it was snow. Also the texture of the snow seemed to be thicker and rather rougher (P 4; M 2, M 6; A 3, A 5).

(A) I was looking for a two-cent stamp in a box of writing-paper. As I flipped over the pieces of paper, the light from an electric bulb filtered in upon the white sheets. I saw these yellow patches of light which flashed by as the pink stamps. Not only were they pink, but they also had roughly the shape of a stamp standing on end—about diamond shaped. I could also see some blurred marks on the stamps which stood for the face. I was certain even when I saw them that I did not have so many stamps, and I am almost as certain now that I saw the yellow of the light at the same time that I saw the pink of the stamps, and the irregular outline of the shadow at the same time that I saw the smoother shape of the stamps. The whole thing was over so quickly that I did not have time to become entirely critical, but I doubted the color and shape even while I was seeing them. The turning of them over had so altered the position of the papers and consequently the size of the patches of light that I was unable to get even the meaning of stamps a second time (P 3; M 2; A 4).

(A) When I opened my eyes this morning I saw a radiator, with six sections, opposite my bed. The radiator was very tall, but I remember seeing only a foot of it, the top foot. I saw the gilt color of the radiator and some rather blurred designs on each section. In a way these sections seemed to be all of the same size, and in a way their sizes seemed to vary. Also there seemed to be colors other than yellow in some of the sections and the gilt wasn't really bright enough. These three discrepancies were enough to make me become very critical toward the experience, and then the meaning 'books' came. The radiator vanished entirely and I found myself gazing at six books, all of different colors and shapes, in a small book rack on the

top of a desk. In all the bedrooms at home, the radiators have the same position in reference to the beds that this desk had to the bed in which I was sleeping. I think the mere fact of waking up and seeing six upright things was enough to give the radiator (P 3; M 2, M 6; A 4, A 5).

(A) An hour before I had been talking to two girls in the hall of one of the buildings. I noticed that they were both wearing brown coats and hats, and, as well as I can remember, I remarked on the fact; I am sure I thought about it. Just now I saw these girls across the campus. I was too far away to distinguish their features, but I did make out their figures and saw distinctly the brown of their coats and hats. One skirt had a fringe on it where it should have been plain. This fact caused me to become critical, and as I did so the strangest thing happened. The girl with the fringed skirt remained as she was, but the other girl changed entirely. Not only did the color of her hat and coat change from brown to blue, but she actually shrank, almost gradually, in from the top, and spread out just as gradually toward the sides. She had been a tall thin girl in a brown coat and hat; now she was a short rather stout girl in a blue coat and hat. I ran to overtake these two people to see who they really were, and when I got almost up to them, I saw that they were not the girls I thought they were, but two girls with whom I was only very slightly acquainted P 1; M 2; A 3; P 1; M 1, M 3, M 6; A 1, A 2, A 5).

(A) I woke up this morning to find that it had rained in the window by my bed. When I got up I looked at the floor under the other window, on another side of the room, and saw there a small puddle of water. I saw the smooth liquid of the water, but I also saw that it stood much too high above the floor, about an inch I should have said. This height of the water worried me, so I went over toward it to examine it better. As soon as I started moving, but after I had started looking very intently at the puddle, the shiny smoothness disappeared and I was staring at a sandal over-shoe (P 3; M 2, M 6; A 3, A 5).

(A) When you look from a certain window of the laboratory you can see, off in the same direction, a road and a smoke stack. The road is about twice as wide as the smoke stack and is seen slanting back from the observer; the smoke stack stands erect. Just now I looked from the window and noted only the smoke stack. It had its proper size and vertical position but appeared too far to the left. Then as I looked at it to see why it was displaced, it spread out and became wider, and seemed to slant back into the hill, and I discovered that I had been looking at the road (P 1; M 2, M 6; A 3, A 5).

(A) Early this morning I heard the chimes playing "In the Gloaming." I wondered why they should play it so early in the morning, but it wasn't until several seconds later, when I noticed a defect in the rhythm, that my attitude toward the piece became in any way critical. Then I heard an entirely unfamiliar melody. The piece was repeated and I listened carefully to see how I could have made the mistake. I found the value of the notes in the first four measures to be almost identical with those of the first four measures of "In the Gloaming," and their pitches to be not so very different, but not at all the same (P 1; M 1, M 2, M 6; A 1, A 5).

(A) The chimes were playing "Seeing Nelly Home." I heard the chorus played straight through once, but after it was over I realized that there had been something wrong with the time; the pitch of the notes had seemed correct. So when the chorus was repeated I listened quite carefully and I found the melody to be strange to me, and that I had not only changed the pitch of most of the notes but had actually quickened some and retarded others, and filled in a few pauses (P 1; M 2, M 6; A 3, A 5).

(A) The chimes started "Deep River." They played for about three measures; then the time became too rapid, and I heard an unfamiliar melody (P 1; M 1, M 2, M 6; A 1, A 3, A 5).

We had chocolate pudding for dinner. Late in the afternoon, I went to the refrigerator and got out a bowl of the pudding. I put sugar and cream on it and began to eat it. I was swallowing the first mouthful when I noticed that something was wrong. Immediately what I had in my mouth tasted salty and greasy and I realized that it was gravy. I cannot say that it had tasted just like the pudding before this, but it had certainly tasted much more like it than it had like gravy (P 1; M 1, M 2, M 6; A 2, A 5).[11]

III. Experiments

Although we have presented our casual instances first in this paper, the experiments which we are about to report were performed simultaneously with the collecting and classifying of these instances. As soon as, from some chance example, we caught the hint of an experiment we worked out and performed this experiment; testing, by varying them, the conditions we already suspected; checking, by a knowledge of the stimulus-object, the nature of the untransformed perceptive experience and the actuality of the overlay; and learning, by using many observers, trained and untrained, trustful and distrustful, more about the overlay itself. It should be borne in mind, for a clear understanding of the latter part of the paper, that these experiments were not performed after the completion of the logical classifications, but that knowledge gained from them aided in the making of the classifications.

The *Os* in these experiments, which extended over the greater part of two years were: Dr. H. P. Weld (W), Dr. K. M. Dallenbach (Da), Dr. L. B. Hoisington (Ho), Dr. H. G. Bishop (Bi), members of the instructing staff; Miss C. C. Braddock (Br), Mr. S. Feldman (Fe), Mr. J. P. Nafe (N), Mr. A. C. Reid (Re), Miss E. Moeller (M), Mr. A. W. Garrison (Gn), Mr. M. G. Scheck (Sk), and the writer (A), graduate students in psychology, all of whom had observed in previous experiments; Mr. F. L. Bixby (By), Miss F. H. Burns (Bs), and Miss D. D. Durling (Du), graduate students with no previous experience as *Os*; and 48 undergraduates. The undergraduates, with a few exceptions, were seniors and juniors who were working on minor problems in psychology or were taking the advanced laboratory course.

A. Natural Objects

Hering[12] and Katz[13] tell of five natural objects, snow, coal, gold, blood and grass, which the memory color falsifies especially. They also offer methods for eliminating the objectivity and

[11] We limit this list to the first hundred cases that came to our notice. The examples might be extended indefinitely. One has only to be engaged upon some intellectual task, such as thinking out a lecture or getting ready for an interview, and at the same time to be set for noticing memory color, —and the memory colors, especially if one moves about while thinking, fairly crowd upon one. We have a report of three marked instances noted within half an hour: after that, the *O* impatiently dismissed the memory color *Aufgabe!*

[12] *Loc. cit.*

[13] *Op. cit.*, 214 ff.

thereby the memory color of these objects; Hering looks at the object through a tube, Katz views it through a hole in a screen. Our first problem of an experimental nature was a study of these overlays by the methods suggested.

(1) *Grass.*—Grass was the first object studied. We wished to obtain, by comparison of the color of the object grass with that of its film, a measure of its memory color. We selected a patch of green grass approximately 8 x 5 m., and in a window directly overlooking this patch set up our apparatus, including a color mixer and a reduction screen, with a 1.25 cm. hole, placed 60 cm. from *O*'s head rest. For eliminating the contrast, which the reduction screen induced on the grass seen through its hole, and the shadow, which it caused on the disc of the color mixer, several alterations in the original arrangement of our apparatus were necessary. Consequently three weeks passed between *E*'s selection of the patch of uniform green grass and the *O*'s first attempts to equate the color of the revolving disc to that of the object grass. Not one of the 6 *O*s, Bi, Br, Ho, N, Re and A, could make a satisfactory match, because, as every one reported, the grass in the limited area was not of uniform tint or chroma. As it was now late autumn, we attributed this irregularity to the effect of frost, and suspended our work until spring. When, in the following spring, *E* noted that the grass was quite green the experiment was resumed. Again the *O*s reported that they could not equate the color of the disc to that of the grass, because the grass in the chosen patch still showed a variety of hues and tints. It was obvious that we must select another patch of grass which should present, under careful scrutiny, an area of uniform green of sufficient dimensions to fill the hole in the reduction screen. Although *E* and the 6 *O*s sought it during the remainder of the spring and during the summer, they were unable to find the requisite area. They found a number of plots, such as nearby lawns or golf courses, some of which had an extent much larger than that required, which were absolutely uniform and very green under casual inspection; but as soon as inspection became to a slight degree critical, the uniformity was gone, and the *O* saw not one green but many. In some cases, a patch, which had been all of one green, even showed spots of brown; but never was the greenest part of a patch, critically regarded, found to be intrinsically greener than the entire area had appeared under the casual attitude.

When, however, fields of grass or grain were so far from the *O* that their areas appeared very tiny, the uniformity of their color usually persisted. These remote fields apparently offered an opportunity for measuring the memory color of grass; and the fields of grain on a hillside two miles from the laboratory, but visible from its windows, were the objects selected for our study. For eliminating the objectivity of these far-away fields, we used a ring 1 mm. wide whose hole was 5 mm.[14] in diameter. This ring was placed on the end of a slender rod which could be attached to the window-sill. In the room behind the window, and placed at an angle of 90° to it, was a grey screen with a square opening slightly larger than the hole of the reduction ring. Through this opening could be seen the disc of a color mixer. The tint of the ring was equal to that of the screen, so that the contrast induced by each was the same. In the experiment the *O* was asked to select the field on the hillside which to him seemed the greenest, and to adjust the ring so that it included this field; and then to equate the color of the disc to

[14]For reducing the color of these natural objects, *E* carried about with her small rings made from the odd numbers of the Hering grey series. The width of these rings was 1 mm., and the diameters of their holes varied, by steps of 1 mm., from 5 to 9 mm.

that of the film and to that of the field seen as an object. The difference between these two matches we were to consider the measure of the memory color.

The *O*s were Bi, Ho, Na, W and A. Observations were made on clear and cloudy days. We never obtained a quantitative measure of the memory color of grass; but our method yielded the following results.

Clear Days.—Three *O*s, A, Bi, and N, saw a marked difference in chroma when a field was seen casually as an object and when its color was seen observationally as a film. Yet a record of this difference was impossible, because the very comparison of the color of the field with the color of the disc necessarily forced the *O* to become critical toward the color of the field; and the color of this field seen critically was practically identical with that of its film seen observationally, though quite different from that of another field seen casually. We append some reports.

A "I selected the greenest field on the hill and adjusted the ring so that almost its entire area was visible through the hole. I matched this film to the disc without any difficulty, and, leaving the match on the mixture, removed the ring so that I could make my second match for the color of the field. I looked at the field carefully to get a good estimate of its color and then turned to the disc. The color on the disc matched that of the field exactly. I was immensely surprised and looked again at the hillside. The field which I had selected had been the greenest on the hill; but when I now looked at it critically, I saw it as less green than the surrounding fields, whose color I still took more or less casually. As soon, however, as I became critical toward these other fields, their color also became more faded. I was able, by saying to myself 'This is the same old field and just as green as ever', to see the rich green again. But as soon as ever I looked at this field for the purpose of estimating its color correctly, this color was quite faded" (P 2; M 1, M 4, M 6; A 1, A 5).

Bi and N were instructed to select the greenest field, but to make their first match to the field, and not to the film, as A had done. They were both unable to make a successful match.

Bi "I cannot match the color which I originally saw. As soon as I become critical toward this field it becomes less green than those surrounding it. I can, by shifting my attitude, let the green return and see it as greener than the surrounding fields, but I cannot hold it long enough to make the match" (P 2; M 1, M 4, M 6; A 1, A 5).

N "That's funny. This field does not look the same color as it did before. It is now less green than the surrounding fields which before were less green than it" (P 2; M 1, M 6; A 1, A 5).

For 2 *O*s, Ho and W, there was no chromatic difference between the green film and the green field.

Under Abnormal Illumination.—When there was a haze of any kind across the valley, so that the hillside was seen under what Katz might term abnormal illumination, all five *O*s found a color difference between a casually inspected field and its film. Again, however, the critical attitude was as effective as the ring in dispelling this overlay; so that again a quantitative measure of the overlay was impossible.

A (rainy day) "I looked out across the valley and all that I could see was a grey mist, darker in some places than in others but with no hue. As soon as I made out the lighter patches to be the familiar green fields, their color was seen as green. This green did not stay long enough for me to make a match, but came and went as I was trying to hold it" (P 2; M 1, M 6; A 1, A 5).

Bi (cloudy day) "There was a decided loss in chroma when I looked through the ring at the field which I had just now seen as quite green; but the green which I now see without the ring is about as poor in chroma as that which I saw through the ring" (P 2; M 1, M 6; A 1, A 5).

Ho (smoke over the hillside) "The field which I selected (casually) was a green of rather poor chroma, but it was definitely green; now (critically inspected) it is a grey with only a hint of green, and the hint gets pretty weak. I really don't believe that I could recognize it as green. It has the color of what I saw through the ring, but there is a difference in texture" (P 2; M 1, M 6; A 1, A 1).

N (cloudy day) "The chroma is not as good as I thought it was. When I selected it, it was the greenest field in sight, and now its color can hardly be distinguished. It is about what I see through the ring" (P 2; M 1, M 6; A 1, A 5).

W (smoke over the hillside) "When I first saw it as field I did not doubt the green, but now there is a question as to whether there is any green there or not. When I looked through the ring, the yellow-green did not come" (P 2; M 1, M 6; A 1, A 5).

The results of this investigation, extending over a number of months, forced us to the conclusion that in our ordinary seeing of grass the memory color serves two purposes.

(1) If the grass is near and would under an observational attitude be mottled, the memory color causes this mottled area to appear uniform; in doing this it does not necessarily make the greenest parts of the area intrinsically greener, but it does make the less green parts equal to the greenest.

(2) When the grass is seen from a distance, the memory color usually enhances the chroma of the green, especially if the conditions are such that under an observational attitude there would be little or no 'color' visible. In both cases, however, a completely critical attitude is adequate to dispelling the overlay, even though the meaning of grass remain.

(2) *Snow.*—The problem, method and results of our experiments on the memory color of snow are so similar to those of the experiments on grass that a detailed account is unnecessary. Although we watched diligently for two winters, we did not find an area of snow, large enough to make color-matching practicable, which would remain completely uniform under critical scrutiny. The change to irregularity with a slight shift of attitude was perhaps more striking here than with the grass, and warrants our quoting a typical report.

A "I looked from the window of the laboratory and saw almost the entire campus covered with soft white snow. This uniformity lasted only a few seconds, however; for as soon as I looked more closely to decide which was the best spot to include in the hole of my screen, it broke up into irregular stretches of yellow between large patches and small streaks of blue. It would have been useful to have got a disc to match these blues and yellows, but this was impossible, because they showed further differences in tint. The yellow in some places was brighter than our whitest paper" (P 2; M 1, M 6; A 1, A 5).

The differences in hue were less on cloudy than on clear days; but even then differences in tint, due to the drifting of the snow and the uneven surface of the ground on which it lay, were noticeable as soon as the attitude became the least bit critical. Even immediately after a heavy snow, when these differences were greatly diminished, color-matching was unsatisfactory because of the rough texture of the snow.

Snow seen from a distance also often showed a variety of tints when critically regarded, as grass did not. The critical attitude always overcame the whitening effect of the memory color on distant snow, just as it cancelled the greening effect of the memory color for distant grass. Therefore no matches were obtainable, and as the reports were similar to those of the preceding investigation, we shall omit them.

(3) *Coal.*—We had been observing coal, in bins, in cellars and in heaps out-of-doors, for three months, before we found a trace of a memory color. The coal which we examined during this time did not change in color when seen casually, critically or (by adjustment of the ring) observationally. The first memory color for coal was discovered out-of-doors on a cloudy day. It was seen by A and two untrained *O*s.

A "I saw a heap of coal about fifty feet away. It was all of a uniform dark grey. I adjusted the ring so that it inclosed an area in the very middle of the heap. This area was much lighter than any of the heap had appeared in the earlier casual observation. I then took away the ring and looked critically at the entire heap. It was no longer a uniform dark grey, but was much lighter in some places than in others. I had happened to get one of these lighter places inclosed in my ring, and its tint was the same through the ring and without it when regarded critically" (P 2; M 1, M 6; A 1, A 5).

P "I thought the pile was all the same color when I first looked at it; but now, since I have seen the middle through the ring, I see that it has many shades" (P 2; M 1, M 6; A 1, A 5).

A continued her observations throughout the year. Although she had many experiences similar to the one quoted above, in no instance did she find the critical attitude failing to break up the uniformity of an apparently uniform heap of coal; nor, when the uniformity was broken, had the memory color intrinsically blackened the darkest part of the heap. The coal was never given in areas large enough to be examined from a great distance; but the results of the observations of coal seen at close range bore out the result of the investigation of grass, that one of the effects of the memory color is to make uniform familiar objects which under a critical attitude show irregularities. The effect of overlay was again to level up rather than to average.

(4) *Gold.*—The roof of the tower of the University Library aided in our study of the memory color of gold. This roof is made of slightly pinkish yellow tiles, of moderate chroma; but a number of the faculty and students take it to be gilt, and see it with a metallic lustre. *E* asked members of the department, and the girls in the house in which she lived, the color of the roof. Those who replied 'gold' and described the color and lustre, she asked to watch for it afterward and be sure. The reports of the trained *O*s, while agreeing with one another, differed from those of the psychologically untrained. We quote one report from each group.

N (trained) "I was on the East side of the tower at eight o'clock this morning and the sun was shining brightly on that side. When I first looked at the roof, it blazed out as solid brilliant yellow gold, all of a piece. Then I looked more carefully, as you had asked me, and it was nothing more than a pinkish buff with no metallic lustre. It seemed to be made of great heavy tiles which did not fall together very well and between which were big heavy seams" (P 2; M 1, M 6; A 1, A 5).

O (untrained, facing a shaded side of the roof) "It isn't gold from here, but a yellowish grey; but if you come around to the other side you will see it look just like gold. I have been watching it since you asked me to, and yesterday it was a perfect yellow gold. I showed it to the person I was with and he also saw the gold." Again (facing a side of the tower on which the sun was shining): "No, it isn't gold from here either, though it is more like it than it was on the other side. I am absolutely certain that it did not look like this yesterday but was perfect gold" (P 2; M 1, M 6; A 1, A 5).

Again the critical attitude was equal to dispelling the overlay.

(5) *Blood.*—Our procedure for studying the memory color of blood was simple. *E* washed the dorsal side of her left fore-arm with soapy water until the epidermis was soft. Here, about 10 cm. above the wrist, she drew

a thin irregular lightish brown line, about 4 cm. long, letting the light brown ink spread into the tiny grooves of the skin. On the same side of her dress she fastened the guard to a fraternity pin; the opened pin, which was fastened to the guard, dangled loosely with the point showing. The loose pin caught the attention of any one with whom *E* talked. When this person told *E* her pin was unfastened, *E* glanced quickly from the dangling pin to her arm and, exhibiting the scratch, exclaimed: 'Oh, it scratched my arm.' After the *O* had examined the 'scratch' *E* covered it with her handkerchief and demanded a description of the blood. When this was finished, *E* removed the handkerchief, and requested the *O* to take a critical attitude over against the mark on her arm and to describe the color and texture of what he saw.

Results. Four *O*s, Ho, N, Re, and Si, an undergraduate, found changes in color and texture, when the scratch was examined critically. All reported that the critical seeing of the scratch showed a smooth light brown line; but two reported that first they had seen a bright red scratch, and two that they had seen a dark red clot.

Ho "I saw it as a scratch of bright red blood. I saw a smear of blood. It was difficult not to see it as blood even when I examined it closely; but under careful scrutiny it changed color very quickly, as if it changed under my eyes without my actually seeing the change. Under this careful scrutiny it was light brown and smooth" (P 2; M 1, M 6; A 1, A 5).

Re "I saw red glistening blood like that from a fresh cut. I saw it standing up above the surface of the skin and running out into little streaks on either side. I did not doubt it for a moment; and then it changed to a flat brown mark" (P 2; M 1, M 6; A 1, A 5).

N "At first it was dark red and clotted over, like any wound that has stopped bleeding. I could see the raised skin on both sides of it. Now it is a yellowish red and all smooth" (P 2; M 1, M 6; A 1, A 5).

Si "I saw it as blood but not fresh blood. It was red but not shiny. It stood up above the rest of your arm. Now it is flat and brown. The red was darker and thicker than the brown" (P 2; M 1, M 6; A 1, A 5).

From our investigations of the perceptions of the five natural objects grass, snow, coal, gold and blood, we may say that Hering and Katz were correct in claiming that the seeing of these objects is ordinarily affected by a memory color. Although our investigation failed to give us a quantitative measure of a single memory color, it was thus not unfruitful.

B. Controlled Experiments: the Overlays in Perceptions of the Various Sense Modalities

Experiment 1. Smell

Three 'wrongly' perfumed artificial flowers were the objects in our olfactory experiment; a pink rose scented with violet perfume, a red rose with lily perfume, and a bunch of violets with lily perfume. The *O*s, who were Br, Fe, N and Re, and 18 undergraduates (3 with some psychological training as *O*s in former experiments, 15 psychologically untrained), did not know until the experiment was completed that they were taking part in a psychological investigation.

Our first method consisted in showing the flower to the *O*, telling him that it was a new kind of artificial flower which smelled as well as looked like the real flower it represented, and asking him if he did not think the imitation excellent. After-

wards the *O* was asked to shut his eyes and to identify the odor presented. Later we altered our method, so that *O*'s interest in the object, not *E*'s question about the odor, should initiate the smelling. In this modified procedure, the flowers were placed on *E*'s table or pinned to her dress. When an *O* noticed the flower, whether he took it to be real or artificial, *E* quite casually told him to smell it.

Results. Only one *O*, untrained, of the 10 *O*s (4 graduate students and 6 untrained undergraduates) serving under the first method, gave unequivocal evidence of the presence of an olfactory imaginal overlay. Of the 12 *O*s, serving under the modified procedure, 2 gave no evidence of such an overlay. A brief summary of the results of the 10 *O*s (Ba, Fu, and Sm with psychological training, the remainder untrained) is as follows.

Bc smelled the rose with the violet perfume as rose, the rose with the lily as rose, and the violets with the lily as violet. With eyes closed, she could identify both odors correctly (P 1; M 1; A 1).

Bn. Same as for Bc.

Bt smelled the rose with the violet perfume as rose; also the rose with the lily as rose, but reported that while she was smelling it the odor changed to 'something unfamiliar'; the rose with lily, she said, smelled as the violets had done after the odor changed. With eyes closed, she could identify the violet, but not the lily perfume (P 2; M 1, M 6; A 1, A 5).

Ba smelled the rose with the violet perfume as rose; and then reported that, when he realized the flower was artificial, the odor changed to that of violets. He identified the lily perfume on both of the other flowers (P 2; M 1, A 6; A 1, A 5).

Bk smelled the rose with lily perfume as rose, and then reported that the experience changed to lily-of-the-valley. She identified the two odors on the other flowers (P 2; M 1, M 6; A 1, A 5).

M smelled the rose with lily as rose. She suspected it was an experiment after this, and got no overlay for the other flowers. She could identify both odors correctly (P 2; M 1, M 6; A 1, A 6).

Fu first smelled the violet with lily as violet, and then reported that the odor became that of a lily. She named the lily and the violet of the two other flowers (P 2; M 1, M 6; A 1, A 5).

I smelled the rose with lily as rose, and then the experience changed and became unfamiliar. She reported that the odor of the two other flowers was also unfamiliar, and she could not identify either one with closed eyes (P 2; M 1, M 6; A 1, A 5).

L smelled violet with lily as violet, and then said: 'It's changed to lily.' She identified correctly the odors of the two remaining flowers (P 2; M 1, M 6; A 1, A 5).

Sm smelled the rose with violet as rose, and then reported a change in the experience, which she later identified as the odor of violets. She identified the lily perfume on both the other flowers (P 2; M 1, M 6; A 1, A 5).

The difference in result under the two methods demonstrates the necessity of that condition for the appearance of the overlay which in our classification we called the *casual attitude;* for with one exception (the untrained *O* who described three apparently adequate overlays) every *O* serving under Method 1 reported a tendency to doubt *E*'s statement about the odor and to detect something 'wrong' in it. This sceptical disposition we

may consider the extremely critical attitude. The changes in the olfactory experience reported by the *O*s serving under Method 2 indicate a shift from a completely casual to a more or less critical attitude. This change occurred during the sniffing of the flower, usually when a flower which was thought to be real was found to be artificial.

Fu "I saw the violets in the vase on the table, and thinking they were real leaned over to smell them. As I got very close I noticed the clothy texture of the petals and realized that they were artificial. I sniffed at them just the same and for the first instant they smelled just like freshly plucked violets; then their odor changed to lily" (P 2; M 1, M 2, M 6; A 1, A 4, A 5).

After an *O* had once adopted this doubting or questioning attitude toward the odor, he reported no more overlays.

Experiment 2. Tone

This experiment was carried out in four adjoining rooms of the laboratory. In one room *E* beat out three familiar melodies ("America," the Cornell "Alma Mater," and the "Wedding March" from *Lohengrin*) upon a spiral gong, whose sole note had the pitch of the F below middle C.

Method 1. The *O* had been asked to observe in an experiment whose object was the determination of the intensive limen for melodies. His instructions were: "Listen for a melody which will be played in the next room. If the notes are strong enough for you to recognize the melody, name it. Then move your chair back two feet and listen for the next melody."

Method 2. The *O*s were uninstructed, and were supposedly observing in other experiments. They listened occasionally to the tonal experiment in the next room and later reported to *E* what they heard.

With the first procedure there were 14 *O*s, 2 graduate students, Fe and Re, and 12 undergraduates taking advanced laboratory courses in psychology; in the second procedure there were five *O*s, one graduate student, Br, two seniors with psychological training as observers, Ds and K, and two students in the advanced laboratory course, C and Ja.

Results: Method 1. Only 4 *O*s, all undergraduates, heard the melody ascend and descend in pitch, and these heard the change only in the first melody.

Bec "I recognized the first tune as America and I thought it was being played upon tuning forks of different pitches. When I heard the second piece (Alma Mater) I could not recognize it immediately; and while I was trying to figure out what it was I realized that the same note was being played over and over. I think the procedure must have been changed between the first and second pieces" (P 1; M 1, M 6; A 1, A 5).

Ber "In the first piece I somehow had an idea that you were just using one note, but still I heard the melody go up and down. In the second mel-

ody I was certain from the very first that there was no change in pitch" (P 1; M 2, M 6; A 4, A 5).

Ry "I heard the melody go up and down in the Wedding March, but I felt there was something funny about it, I didn't know what. In the second tune (America) I saw that it was all the same note" (P 1; M 2, M 6; A 4, A 5).

Sm "I thought I heard changes in pitch for the first two measures of America, but when it did not go as it should in the third measure I realized it was all being played on the same note. I heard no change in pitch after this" (P 1; M 1, M 6; A 1, A 5).

Method 2. All five *O*s heard some change of pitch.

Br "At first I realized that some kind of a tone experiment was being carried on in the next room. I do not know whether I heard rise and fall of pitch then or not. Suddenly I heard "God Save the King;" and I remembered beating it out as a child. Just then I heard it as all one pitch, but I said to myself "Is it all one pitch?" and I immediately heard it go up and down. It went neither as high nor as low as it should, but sounded as if you were playing it on certain notes, one for high, one for low and one for medium. Then I wondered if there were three notes or only two, and immediately I realized that there was only one" (P 1; M 3, M 6; A 2, A 5).

Ds "I recognized all three tunes, but the range of the notes did not seem to be great enough. The middle notes were all right, but the higher notes seemed to be flat and the lowest ones not low enough" (P 1; M 3; A 2).

K "I heard the three tunes, but there was something queer about the pitch of the notes. That's why I wondered what kind of an instrument you were using" (P 1; M 3; A 2).

Ja "My partner and I heard the Wedding March and wondered who was playing it. After that I heard no more melodies, but one note over and over" (P 1; M 1, M 6; A 1, A 5).

These results give us descriptions of two of our partially adequate overlays; the conflict and the compromise. They also show, those from Method 1 in particular, how this partial adequacy sooner or later forces the *O* to become critical. A comparison of the results of the two methods demonstrates once more the necessity of an entirely casual attitude for the appearance of an overlay. Method 1 did not necessarily cause the *O* to doubt the change in pitch of the melody, but it did require him to listen carefully to the melody itself. This attitude was therefore more nearly critical than that induced by Method 2, but evidently not so critical as that induced by the questions in Experiment 1.

Experiment 3. Taste and Smell

Our materials were the Minoras (chocolate mint wafers) and Anolas (chocolate flavored sugar wafers) confused in one of the casual instances.

Method 1. *E* told her *O*s in Experiment 7 that, to repay them for their kindness in observing, she would give them some peppermint wafers of which she was especially fond. For ten successive observation periods she offered the *O* a box of Minoras, was insistent that he take more than one, and nearly every day spoke of the peppermint flavor. On the eleventh day

the box which she offered the *O* contained Anolas. She asked him to describe how the first wafer had tasted and how the second wafer was now tasting.

Method 2. Upon opening a cupboard in the laboratory *E* expressed surprise at finding a plate filled with wafers (Minoras). Bi, Sm and Ber who were near were really surprised to see them. *E*, being unable to find to whom the wafers belonged, proposed that they four eat some; and as she bit into one exclaimed, "Why it is flavored with peppermint!" The following day the Minoras still on the plate were replaced by Anolas. At separate times *E* suggested to Bi, Ber, and Sm that they see if the peppermint wafers were still there; "if so, let's eat some." *E* asked for a description of the flavor of these wafers.

A also was an *O* in this experiment.

Results. Method 1. (*a*) By and M declared that the flavoring of the wafers was just as it had been on the preceding days. By ate only one wafer, but M ate three, and insisted that she could taste the peppermint in each one (P 2; M 1; A 1).

(*b*) Gn and Sk each thought that the peppermint in his first wafer was weak. With his second wafer, Gn reported that he could alternately taste and not taste the peppermint; but Sk continued to taste weak peppermint in his second wafer (P 2; M 3; A 2, A 4).

(*c*) Bs and Du tasted only chocolate (P 2; M 6; A 5).

Method 2. (*a*) Ber reported that her first wafer was just as 'peppermint' as those she had eaten the day before, but that the second one, eaten after *E*'s question, was quite different (P 2; M 1, M 6; A 1, A 5).

Bi reported that when he first tasted his first wafer it tasted decidedly minty, but that while he was eating it he wondered if there was some trick, and the peppermint taste immediately disappeared (P 2; M 1, M 6; A 1, A 5).

(*b*) For Sm, both wafers that she ate, one before and one after being questioned, tasted somewhat like peppermint, but not as much so as those eaten the day before (P 2; M 3; A 2).

A's report is as follows: "I was of course very familiar with the taste of the Minoras, as I had eaten one or two with each of the *O*s. This morning I felt hungry and reached absently for a wafer. The wafer that I picked up seemed to have a brown filling and I judged it to be an Anola. The first bite was merely chocolate; but I wondered if, by making myself believe that it was a Minora, I could make it taste of mint. And the second bite of the wafer tasted of peppermint, but only faintly. The curious thing was that this weak peppermint lasted. This led me to look carefully at the wafer; and I then saw that its filling was white. The third and last bite was more strongly peppermint, but even then not as strong as it should have been. The second wafer that I took from the same box, which I found to be marked 'Minora', was distinctly peppermint" (P 2; M 1, M 2, M 6; A1, A2, A5).

A's report describes an adequate overlay and its gradual disappearance, similar to that reported by Fu in Experiment 1. The similarity of results under the two different methods demonstrates that one impressive experience with an object is as efficient in stamping in an overlay as many indifferent experiences with the same object. In Method 2 the extreme famil-

iarity of the situation kept *E*'s question about the taste from forcing the *O* into a completely critical attitude toward it. Here we have evidence of the close relation between perfect familiarity and the casual attitude.

Experiment 4. Weight

With Sir George Pearson's experience with the potassium in mind, we devised experiments for studying the imaginal overlay of lifted weights.

Method 1. A wide-mouth glass vial, 7.5 by 3.75 by 1.25 cm., bearing the label *Hg*, was lined at the bottom and half way up the sides with tin foil, and filled to the height of the tin foil with water. Had the bottle been half filled with mercury it would have weighed a pound; with the water it weighed 4 oz. We placed it among the tools on the workshop bench. Bi, Br, N and Re, finding it in their way and moving it, were asked to report on their experiences when lifting it.

Method 2. The covers of Titchener's *Text-Book of Psychology* and *A Beginner's Psychology* were filled with basswood so that in form and size they were identical with the ordinary page-filled books. Their weights however were different; the real *Text-Book* weighs 26 oz., the false one weighed only 14; the real *Beginner's* 19 oz., but the false one 11. These false books were substituted for the real on the shelves of their owners. Unless the *O*, having lifted the book on his own account, volunteered a description of his experience while lifting it, *E* asked him to lend her his *Text-Book* or his *Beginner's*, and after he had handed it to her demanded this description.

Results: Method 1. The lifted mercury failed to reveal the presence of an overlay in the experience of any of the 4 *O*s. Every one reported that he realized, more or less clearly, that the mercury would be heavy, and that he was therefore 'set' to pick up a heavy object. The lightness of the weight he noticed immediately and suddenly, as soon as he began to lift the bottle.

Method 2. (*a*) Bi, Da, and W were unable to report on their kinaesthetic experiences while lifting the books. All three reported that their attention was directed not toward the feel of the book, but toward the sight of it and the fact of lending it to *E*. All three were surprised to find how light the false books really were.

(*b*) Br and Fe described experiences which we, with certainty, can designate as overlays.

Br "I took the *Text-Book* from the shelf to look up a reference. Because it was heavy I rested it on the desk while I opened it. When I found that it was wood I lifted it again, and it was light, but not as light as I

thought it should be. I picked up another book of the same size, noted its heaviness, and then took up the *Text-Book* again. This time it was much lighter than it was the second time I lifted it" (P 1; M 1, M 2, M 6; A 1, A 2, A 5).

Fe "I saw that the *Beginner's* was made of wood while I had it in my hand. Before that it had felt, so far as I can say, as the real book should, and even then it was still heavy. Then as I continued to feel it, it very gradually became lighter and lighter" (P 1; M 1, M 2, M 6; A 1, A 2, A 5).

(*c*) Ho detected the lightness in weight as soon as he began to lift the *Text-Book*.

A comparison of the results from the two methods indicates that the overlay is more effective when the difference between it and the untransformed experience is small than when it is great.

That Bi, Da and W were unable to report on any overlay because of the relative unclearness of their kinaesthetic experiences is further proof that our interpretation of Hering's "not paying especial attention" as a casual attitude, and not as a lack of clearness, was correct. Yet there was probably something imaginal in the experiences of Bi, Da and W which prevented their noting, as did Ho, the extreme lightness of the books; for their familiarity with them was no less than Ho's.

Experiment 5. Pain

The instructions to the *O*s, who had been asked to observe in an experiment which should determine the limen of subcutaneous pain, were: "The pressure on the palm of your hand will be gradually increased. Watch the pointer, and report the place where you feel pain. Do not wait for intensive pain, but report the first positive change in the quality of the experience." The pointer of the algometer, which exerted the gradually increasing pressure for the first four observation periods, was very large, and the white face over which it moved was marked with heavy black numbers. For these four days, five values were taken for each hand. In the fifth observation period, which occurred two weeks after the last of the preceding four, the *O* was reminded of his approximate limen. The procedure was the same as before; but the spring had been removed from the algometer, so that its pressure, after it was first placed upon the hand, remained constant at a point considerably below that of the pain limen.

The *O*s were Re and 8 students from the course in quantitative psychology.

Results. (*a*) Four *O*s reported the change as soon as the pointer reached the critical mark. They all described this change as being identical with the changes on the preceding days.

(*b*) Three *O*s reported a few 'faint' or 'weak' changes; then 'no change'.

Ba "Same quality, but not as intense as before" (P 3; M 3; A 2).

Gs "I must be sleepy to-day. I feel the change, but it is weaker than usual." This report was for the five trials on her right hand; she reported 'No change' for the left hand (P 3; M 3, M 6; A 2, A 5).

Re "An indistinct something, but not much like pain." "Faint, but otherwise all right." After this he reported that the experience did not change, despite the movement of the pointer (P 3; M 3, M 6; A 2, A 5).

(*c*) Four *O*s reported that they felt no change in their experiences, although they saw the movement of the pointer.

The reports cited under (*b*) again afford a description of a partially adequate overlay (compromise); and the change to inadequacy apparently indicates a growing criticalness of attitude occasioned by this partial adequacy.

The similarity of this experiment to the experiments reported by Edwards in his study of Sensory Suggestion [15] leads to a comparison of the two. We note one fundamental difference between Edwards' general method and our own. The attitude of his *O*s was by hypothesis observational, and the images which they described were evoked by an intruction which *E* gave them while under this attitude. The smelling, listening, tasting, and lifting of our *O*s in the 4 experiments just reported were of necessity object-consciousness smelling, listening, tasting, and lifting. Their images were not grafted on to their experiences by a formal instruction, but came to them as a given and integral part of a familiar object. Even in the test series of this experiment, though we did instruct our *O*s that the pressure would be increased and implied that the quality of the experience would change, we relied for our imaginal overlay not upon the instruction but upon the familiarity of the total situation, including the sight of the rising pointer and the black numbers. This situation we assumed to be an object-consciousness situation. Yet it is barely possible that on the test day our *O*s took an observational attitude toward the tactual experience, and that the change in quality was brought about solely by the instructions; just as, in some of Edwards' experiments, the image which his *O*s describe might have been produced by the recognition of a familiar object and not by observational instructions.

Experiment 6. Touch

The materials were eight squares of cloth,—two velvet, two silk, two cotton, two serge,—all with both sides identical, folded over and sewed so that they were all of equal thickness.

Method 1. The *O* sat at a table on which was a narrow board covered over with a black cloth. *E* gave him these instructions: "Before you are four squares of material. They are velvet, silk, cotton and serge. Begin with the left hand square and try to identify them by feeling them between your thumb and finger. When you are sure of any sample, thrust it over the board. If you are sure at the first attempt, throw out the sample at once. If you are not sure you may test the samples over and over, beginning always at the left and going straight through to the right. Close your eyes, and when I say 'now'

[15] A. S. Edwards, *An Experimental Study of Sensory Suggestion*, this JOURNAL, xxvi, 1915, 99 ff.

begin to feel the left hand sample." There were always four samples on the board, but never, under this procedure, more than three different kinds of material; every set therefore contained two identical samples. These were never placed next each other. Every set contained a new substitution and offered a different succession of the samples.

When the *O* had named all four samples and thrust them over the board, *E* covered them again with the cloth and asked *O* to open his eyes and describe the feel of each sample which he had named. This procedure was continued until the *O* was suspicious of the method and failed to give any evidence of the presence of overlays in his experience. Then Method 2 was adopted.

Method 2. The instructions were the same as before; but, for the first six observation periods, samples of all four materials mentioned in the instructions were actually presented. In these practice series the *O*, after he had identified the four samples, was allowed to feel each sample as long as he wished, with his eyes open. In the seventh observation period there was a 'substitution series' which contained duplicate samples. Whatever the result of this series, the *O* had a 'practice series' in the next observation hour; but in the hour after that, a different 'substitution series'. This procedure was continued until every material had been substituted for every other material. The number of practice series between successive substitution series varied from one to four; and reports of the feel of the cloths was required for the practice as well as for the substitution series.

The *O*s in both procedures were Bi, Fe, N and Re; 6 students, Ba, Ber, Gi, He, Ja and Ru, with minor problems in psychology and with some training as *O*s in other experiments; and 5 undergraduates, Ad, Al, Bu, De and Je, taking the course in quantitative psychology.

Results: Method 1. Only one *O* (De) failed to report any evidence of an overlay. The distribution of the overlays which appeared in the reports of the 16 other *O*s was as follows. (1) In 13 cases (cotton identified as serge 6: Bi 1, Ber 1, Fe 1, Je 1, Gi 1, N 1; serge as cotton 6: Ad 1, Ba 1, Bu 1, Fe 1, Je 1, Re 1; silk as velvet 1; F) the overlay was apparently quite adequate; the missing sample was named and its feel described. This feel evidently came when the *O* first touched the sample and persisted until he thrust it over the board, for the *O* never doubted its reality or the statement that there were four different materials before him.

Gi (cotton as serge) "Cotton had dull feel. Serge was rough" (P 2; M 1; A 1).

Bi (serge as cotton) "Number 1 felt woolly and rough, and what I felt as woolly made me say serge; number 3, the cotton (serge), when I took it

up was rough, but did not feel woolly. The serge was softer than the cotton and tended to compress between my fingers more than the cotton" (P 2; M 1; A 1).

Fe (silk as velvet) "Silk is smooth and hard. Velvet is smooth but yielding" (P 2; M 1; A 1).

(2) Twice an adequate overlay was late in coming.

He (cotton as silk) "Silk felt different from cotton on second feel. Didn't have slick feeling for a moment, but then the slick feeling came and I felt that they were different" (P 2; M 6, M 1; A 5, A 1).

(3) In seven instances the overlay was only partially adequate, but this partial transformation was stable (serge as cotton 4: Al 1, Ja 1, N 1, Ru 1; cotton as serge 2: Ja 1, Ru 1; cotton as silk 1: Je).

Ja (serge as cotton) "These two felt much alike, but I felt the rib in the serge and not in the cotton. The cotton had a smoother feel and the serge felt more loosely woven" (P 2, M 2, A 2).

Ru (cotton as serge) "Had a hard time distinguishing cotton and serge. Cotton seemed coarser, while the serge had ridges in it and felt somewhat finer than the cotton" (P 2; M 2; A 2).

(4) Three times, however, this partially adequate overlay disappeared between successive feels of the sample which it transformed (Re cotton as serge 1, serge as cotton 1, silk as serge 1).

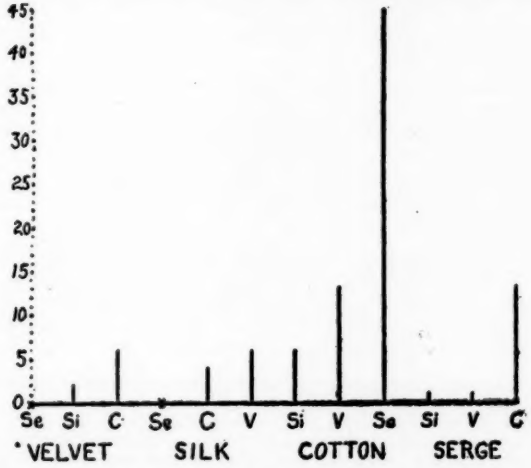
Re (cotton) "At first feel, I thought that the weave of the first was heavier and a little larger, and it felt a little harder, I think. This was my first impression, but the second time I couldn't tell any difference" (P 2; M 3, M 6; A 3, A 5).

Method 2. The effect of the practice series on the attitude of the *O*s was different. Seven *O*s, realizing that they had made 'mistakes' in identifying the samples, resolved not to be 'fooled' again. Their attitude became very critical, and the practice series tended to heighten this criticalness. They reported no overlays in either the 'practice' or the 'substitution' series. Yet the practice series recaptured the confidence of 9 *O*s (Al, Ber, Bi, Fe, He, Je, N, Re, Ru), who thought that *E* was really trying to help them in identifying the samples, and reestablished their casualness of attitude. They reported overlays in the practice as often as in the substitution series, and described those which disappeared and were recognized as imaginal as readily as those which persisted and were mistaken for sensory.

The chart shows the contamination of each actually presented sample by the overlay of another sample, without regard to the adequacy of the overlay or the amount of transformation.

In regard to adequacy, these 97 overlays showed the following distribution.

(1) In 13 instances the transformation of the perceptive experience by the overlay was complete and permanent (cotton by serge 5: Fe 3, Je 1, Re 1; silk by velvet 3: Fe 2, Re 1; cotton by silk 2: Fe 1, He 1; silk by cotton 2: He 1, N 1; serge by cotton 1: Je). The criterion of adequacy was not alone that an actually presented sample was named and described as a material totally different, but also that (*a*) two different materials felt in immediate succession were reported as feeling entirely differently, or (*b*) two different materials felt in immediate succession were described as identical, or (*c*) the *O* was so sure that he was feeling the familiar material that he remarked on the fact.



The ordinate represents the number of cases. The abscissa represents the overlay: the initials, the quality of the overlay; and the full names, the material contaminated.

(*a*) Ge (two serges) "Cotton had fine texture and was yielding to the touch. Serge was less yielding to the touch and I could feel the individual fibres" (P 1; M 1; A 1).

(*b*) Fe (cotton and serge) "The second and third felt just alike I felt the first one and said to myself 'rough, serge'; then the third one felt just like it" (P 1; M 1; A 1).

(*c*) N (silk and cotton) "The first time I felt the two cottons they both felt a little like serge. The second time they were normal again" (P 1; M 1; A 1).

(*d*) Re (cotton, silk, serge and silk called cotton, velvet, serge and silk) "The cotton at first felt a little like velvet; it seemed that I could get the springy texture of the velvet. But as soon as I felt the real velvet I was sure" (P 1; M 1; A 1).

(2) In 10 instances an adequate overlay faded before the *O* could name its material (cotton by serge 6: N 5, Re 1; cotton by velvet 2: He 1, Re 1; silk by velvet 1: Fe; serge by velvet 1: He).

He (serge) "Felt like velvet and then it felt too hard and too cold for velvet. For the first instant I felt the pile and the softness of it; then this gave way to hardness and I recognized it as serge. The softness went out instantly" (P 1; M 1, M 6; A 1, A 5).

N (cotton) "I was looking for serge and I thought I had it. I would have said serge if I had had time to get the word out, but before I could get the word out it lost its roughness and became harder. The roughness melted right away. Then I looked for the softness and it was gone. The melting was gradual but quick. I didn't feel it as rough and then as smooth, but felt it rough and the roughness gradually but very quickly changed to smooth. Then it was cotton" (P 1; M 1, M 2, M 6; A 1, A 2, A 5).

(3) The 74 partially adequate overlays can be divided as follows.

(*a*) In six instances the transformation was so nearly complete that the sample was named from the overlay, but was interpreted as a different grade of the same material (cotton by serge 4: Ber 2, Ru 2; serge by cotton 2: He 1, Ru 1).

Ru (serge) "Another cotton. Did not feel like the other cotton, was more rigid. Still it felt more like the cotton I had just felt than the serge of my memory" (P 1; M 2; A 3).

(*b*) In 8 instances, where the transformation was less complete but still permanent, the sample was again interpreted as a new one, but this time it was described as resembling equally two of the familiar cloths (cotton by serge 5: Al 1, Ber 1, Bi 3; cotton by silk 1: Je; silk by velvet 2: Bi).

Ber (cotton) "I haven't felt this one before. I should say it was part cotton and part serge" (P 1; M 2; A 3).

Bi (cotton) "Not warm, fluffy or scratchy enough for serge; on the other hand more fluffy and more scratchy than cotton. If I rub it lightly it is fluffy and soft. When I compress it the fluffiness yields. Could not feel any weave; it is smooth. Heavy like cotton; the texture is like the serge. Neither cotton nor serge, perhaps a shortnap cotton-flannel" (P 1; M 2; A 3).

Je (cotton) "New material. Texture not coarse enough for cotton and too slippery, yet not smooth as silk" (P 1; M 2; A 30).

Bi (silk) "Velvet silk; sticks like velvet, slips like silk" (P 1; M 2; A 3).

(*c*) In 27 instances the imaginal contamination was so slight that the sample was correctly named; yet the overlay came as an integral part of the tactual experience and its effect was constant (cotton by serge 8: Al 1, Bi 2, Je 1, Re 3, Ru 1; cotton by velvet 6: Bi 2, Je 1, Re 1, Ru 2; cotton by silk 3: Bi 1, Je 1, Ru 1; serge by cotton 3: He 2, Re 1; velvet by cotton 2: Al 1, Re 1; velvet by silk 3: Al 1, Bi 2; silk by cotton 1: Re; serge by silk 1: Ru).

Al "Cotton felt as if it might have a little serge in it, just a little" (P 1; M 2; A 3).

Bi "Cotton felt a little velvety, like a very short pile velvet; had the softness of the velvet, but the bumpiness and heavy bulk of the cotton" (P 1; M 2; A 3).

Re "Cotton had smooth silky finish, but not enough to be silk" (P 1; M 2; A 3).

Bi "Last velvet a little silky; kinaesthetically it was silk, tactually velvet. Had soft slipperiness like silk. Felt different from the first velvet which was heavy and unyielding" (P 1; M 2; A 3).

Re "First silk didn't feel so smooth or so glossy, the second one had more of the real silk feel. The texture of the first one was very fine but not slippery; like cotton in this respect, but not as rough in texture as the cotton" (P 1; M 2; A 3).

(*d*) 33 partially adequate overlays were transitory; their disappearance was in general described as a 'fading' or 'melting' (cotton by serge 18: Al 1, N 8, Re 9; cotton by velvet 5: Al 1, N 1, Ru 3; serge by cotton 5: Re 1, Ru 4; velvet by cotton 3: Ru 3; cotton by silk 1: N 1; silk by velvet 1: He 1.

Re "When I first felt the cotton, I had the tactual experience of serge twills. I felt the rough surface of the serge. Then it faded out; not fully, immediately, but I could feel it getting less and less. When I identified the sample as cotton, I discovered that the twills weren't there at all" (P 1; M 1, M 2, M 6; A 1, A 3, A 5).

This complete disappearance of an overlay usually left behind it the untransformed experience, but not always. The perceptive experience was sometimes retransformed, partially or completely, by a second overlay.

Al "When I first felt the cotton, it had a little velvet in it, just a little. This was just at first, then it changed and felt more like serge" (P 1; M 2; A 3).

He "The silk (which was in fact cotton) had a little bit of the texture of the velvet for a moment. I felt it again and could not detect the velvet" (P 1; M 2, M 1; A 3, A 1).

N "When I first felt the silk (which was in fact cotton) it felt softer than it should, more like velvet. Then it became normal again and just like the other silk (the real silk)" (P 1; M 2, M 1; A 3, A 1).

In this experiment, which was more thorough than any of those preceding it, we secured a confirmation of the more important results of these earlier experiments. The destructive effect upon the overlay of the critical attitude, first manifest in its extreme form in Experiment 1 as a doubting, and modified in Experiment 2 to a careful attending, was confirmed by the negative results of the seven *O*s whose attitude remains critical during the whole of Method 2. Yet the check of the condition of familiarity upon this attitude, first shown in Experiment 3, was further demonstrated by the descriptive reports of the 8 *O*s whose confidence was reestablished by the practice series. These 97 descriptions, while they are of necessity descriptions of integral components of perceptive objects, are as truly phenomenological as perceptive reports can be; and they have their

analogies in the reports of the former experiments. Furthermore the results of Method 2 provided the material for demonstrating the fact, indicated in Experiment 5, that the closer the similarity between the required overlay and the untransformed experience, the greater is the liability of the occurrence of this overlay.[16] From a consideration of these 6 experiments it should be evident that the rôle of the overlay in the perceptions of the various sense modalities is identical; that in every one its appearance is dependent upon the conditions of the object-consciousness, casualness of attitude, and familiarity; that its degree of adequacy accords with the favorableness of these three conditions and the similarity between it and the untransformed experience; that, with a change in any of these conditions, there will be a corresponding change in the perceptive experience itself; and, finally, that if this change consist in the disappearance of the overlay the disappearance will, in general, be described as a 'fading' or a 'melting'.

Experiment 7. Color

In this experiment we hoped to secure an objective measure of a visual overlay, or true memory color, and a better psychoogical description of this overlay than we had hitherto obtained.

The experiment was conducted in a dark room, 380 x 227 cm. On one of the narrower walls of the room hung the stimulus object; directly across from it, and attached to a shelf built out from the other narrow wall, was a color mixer; midway between the object and the mixer, the *O*'s swivel chair was fastened to the floor. All three were a little to one side of the long axis of the room. Slightly to the other side of this axis were two white reflectors on standards, separated from each other by 1 m.; one of them (Light 1) was 1 m. from the mixer and turned toward it at an angle of 110°, the other (Light 2) was equally distant from the object and was turned toward it at the same angle. When these reflectors were fitted with bulbs, each illuminated one half of the room. Each was attached to a double switch, by means of which either or both lights could be turned on.

In every experiment, Light 1 was on when the *O* came into the room, Light 2 was off, and the disc of the color mixer was covered by a grey screen. *O* saw the glass of Light 1 only when he entered the room; never during any of these experiments did he see the glass of Light 2 or, except when it was in rotation, the disc of the color mixer. At the beginning of every observation period, 2 min. were allowed for the *O* to become adapted to the illuminated portion of the room.

Preliminary Experiment A

The stimulus object was a conventional jar-shaped figure, 41 cm. tall and 42 cm. wide at its broadest part. It was made from Denison's "American Beauty" crepe paper stretched over light grey cardboard. Both reflectors were fitted with 200 watt Mazda lamps, and the light from each bulb was filtered by Gage "Daylight" glass.

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[16] A similar—and similarly indeterminate—remark is made, with regard to recognition, by E. Rubin: *Visuell wahrgenommene Figuren*, i., 1921, 27.

The *O*s in this preliminary experiment were: Bs, By, Du, Gn, M and Sk.

The instructions, given orally by *E*, were: "On the wall behind you is a jar. When I say 'ready-now', you are to turn and face this jar; when I say 'ready-now' again you are to turn again and tell me how the color of the disc compares with that of the jar, and I will change the disc to make it more like the jar. Continue to do this until you are sure that the color of the disc matches that of the jar." *E* threw the switch so that both lights were on while *O* was turning, but only Light 2 when he faced the jar and only Light 1 when he faced the disc. Every inspection, of jar and of disc, was of 5 sec. duration. Only one satisfactory match was required in every observation period, but the *O* was warned that this match should be exact for hue at least, and for tint and chroma also if the judgments were not too difficult.

To divert the critical attitude, necessitated by the requirement of an exact match, from the jar (whose color, until the test experiments, remained constant) and toward the disc (whose color changed continually) the *O* was always instructed to look from the jar to the disc, but was never allowed to look directly from the disc back to the jar. The changes in the color of the disc were made in serial order. In the first 5 observation periods, the series were short and the changes were made by units of 2°. After this, the series were all longer, their end-points far from the probable match of the *O*, and the size of the steps was gradually reduced from 15° or 20° to 2°. If, anywhere in a series, the *O* gave a doubtful judgment, he was not allowed, as stated above, to look directly from the disc to the jar as he wished, but was made to wait a few seconds, while *E* made some apparent adjustment of the motor, and was then instructed to look again from the jar to the disc. If he gave a second doubtful judgment, *E* changed the color back a number of degrees and began a new series. If a satisfactory match was found in a comparatively short series, *O* was asked if he would like to 'begin' another series. By means of these 'beginning' series and the apparent blunders which she made in adjusting the colors of the disc, *E* arranged that every *O* should see the jar approximately 40 times in every observation period. Every *O* observed 3 times a week for 25 observation periods; every *O* therefore saw the jar approximately 1000 times in the "Daylight" illumination.

In order that the *O* should maintain the attitude of the object-consciousness established by requiring him to make only one satisfactory match of the jar in every observation period, *E* talked to him about the jar while he was observing, and toward the end of these preliminary experiments made a thin irregular scratch on one of its sides.

The last of these preliminary experiments was completed three weeks before the *O*s observed in Test Experiment 1.

Preliminary Experiment B

The stimulus-object was a 'peacock blue' silk waist; the illumination was the same same as in Preliminary Experiment A; and the method was similar to that used in that experiment. The *O*s, who were N and Re, observed three times a week for two months, and each saw the waist approximately 1000 times. The last of these preliminary experiments was completed nine months before N and Re observed in Test Experiment 2.

Test Experiment 1

The stimulus-object was the jar of Preliminary Experiment A. Light 1 still contained the 200 watt bulb and the Gage glass, but Light 2 contained a 50 watt bulb and its light was diffused by a plain ground glass. The intensity of the illumination of the two lights was the same, but Light 1 still

gave a "Daylight" illumination, while the illumination of Light 2 was yellowish.[17] The jar was the only object in the room which could show the effect of this yellow illumination.

The procedure was the same as in Preliminary Experiment A. The end-point of the series had been judged "too blue" in the earlier experiments.

The *Os* were those who had observed in Preliminary Experiment A. After this experiment they again matched the color of the jar in the "Daylight" illumination for three observation periods before they observed in Test Experiment 2.

Test Experiment 2

The stimulus-object was a jar, identical in size and shape with the jar of Preliminary Experiment A, but made from Hering grey paper no. 37. Light 1, with 200 watt bulb and Gage glass, was on during the entire experiment, Light 2 remained off.

E told the *Os* of Preliminary Experiment A that she wished to find out how well they could see the color of the jar when Light 2 was turned off, and gave them this instruction: "When I say 'ready-now', you are to turn and look for the jar, which will be in its usual place. As soon as you can distinguish its contours, turn back to the disc and tell me how its color compares with that of the jar and I will change it, as I did in the other experiments, until you have found an exact match." The clause about the contours was inserted for two reasons: (1) to insure the *O*'s seeing an object, (2) to divert any criticalness of attitude, occasioned by the novel situation, from the color and to the contours. The instructions given to N and Re were like those given to the other *Os*, except that *E* had first to describe the shape of the jar for them.

The procedure, except that the switch was not thrown, was identical with that of Preliminary Experiment A, and the series was one whose end point had been judged "too red" in these earlier experiments.

Control Experiments: Experiment 1

The object was an egg-shaped figure of approximately the same size as the jars. The illumination was that of Test Experiment 2.

Experiment 2

The stimulus-object was the jar of Preliminary Experiment A, and the illumination was that of Test Experiment 1.

Bi and Ho were the *Os* in each experiment, and in each they were asked to make 10 satisfactory matches of the color visible in that illumination. The procedure for Control Experiment 1 was the same as that of Test Experiment 2, and the procedure of Control Experiment 2, except that the *O* was allowed to look directly from the disc to the object, was like that of Preliminary Experiment A. In both of these experiments, however, the *O* made more than one satisfactory match in an observation period.

After the completion of these experiments, the matches of the various *Os* under the various illuminations were shown to Bi and 4 students in the laboratory. They were asked to name the colors in terms of the color pyramid, and then, if possible, to give them objective names. The psychological term and the most appropriate objective name are included under the 'quantitative' results.

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[17] We wish to thank Professor S. H. Gage, of the Cornell Medical College, for suggesting this method of varying the quality without varying the intensity of the illumination; also for helping *E* to grind the plain glass.

Results: A. Quantitative

(1) "Daylight" illumination

(*a*) Averages of the 25 matches of the *O*s of Preliminary Experiment A:

O	Red	M.V.	Blue	M.V.	Black	M.V.
Bs	248.88	±5.03	97.16	±2.49	13.96	±4.83
By	248.76	±6.43	96.92	±5.28	14.32	±5.05
Du	246.44	±6.21	96.08	±2.90	17.48	±6.22
Gn	251.6	±2.4	95.84	±1.79	12.56	±2.3
M	247.28	±4.0	96.92	±2.48	15.80	±3.79
Sk	247.2	±2.96	97.48	±3.26	15.32	±3.56

These matches were all red purples or "blue fuchsias."

(*b*) As the arrangement of the apparatus in Preliminary Experiment B was slightly different from that of all the other experiments, numerical results can not be given; but both N and Re described the color which they saw 1000 times as a blue green.

2. Yellow illumination

Averages of the controls and matches of the *O*s of Preliminary Experiment A.

O	Red	M.V.	Blue	Black	White	M.V.	Yellow	M.V.
Bi	351.7	± .62	0	0	1.3	±.96	7.0	±1.0
Ho	352.4	±2.32	0	0	0		7.6	±2.32
Bs	330		15	15				
By	332		0	20			8.	
Du	310		30	20				
Gn	298		56	6				
M	335		10	15				
Sk	330		15	15				

The matches of the controls and of Bi were red oranges or "tangerines." Those of the other 5 *O*s were all red purples; Gn's was a true fuchsia, Du's a red fuchsia, and those of Bs, M and Sk were like the color of the flower of the "Indian pink."

3. Lowered illumination

Averages of the controls and the matches of the *O*s in each preliminary experiment:

O	Red	Blue	M.V.	Black	M.V.	White	M.V.	Green	M.V.
Bi	0	18.0	±3.6	288.5	±7.2	47.2	±5.0	6.3	±3.1
Ho	0	21.3	±3.16	283.8	±7.16	36.4	±4.8	18.5	±3.1
Bs	135	52		173					
By	122	80		158					
Du	0	0		335		25			
Gn	78	32		232		18			
M	16	90		204		50			
Sk	20	30		285		25			
N	0	32		265		18		45	
Re	0	28		292		10		30	

The matches of the controls and of Du were greys.[18] The matches of Bs and By were blue fuchsias; Bs's differed only in tint and chroma from her matches in the preliminary experiments, By's was slightly bluer. Gn's match was still bluer and more of a plum color. The matches of M and Sk were lilacs. The matches of both N and Re were poorly saturated "peacock blues."

B. Qualitative

Of the 5 *O*s who served in Preliminary Experiment A and whose matches for the grey jar showed the presence of a memory color, Bs alone reported that the color which she saw in the lowered illumination persisted during the entire observation. She did not once doubt that she was seeing the familiar jar, or that the color which she matched, despite the fact that it "never got all the way out to the edges," was visible in the dim light. The only change which she noted was that, while she looked at it, "it shrank in from the edges and then spread out again."

By, Gn, M and Sk all reported that the color which they saw did not cover the entire jar; and although they also did not doubt that they were seeing the familiar jar, they reported that the color was instable, and that its instability interfered with a successful matching of it. They all voluntarily described this fleeting color. The spontaneous reports of all 4 *O*s included characterizations of size, form, texture, and mode of disappearance. We quote their reports under these characterizations.

Form and size.—By "Doesn't have contours of that thing at all. I can't say how big it is, but it seems to be in the upper left hand corner where I used to look when I matched it."

Gn "Color must be over the entire jar, but I can see it more distinctly in the center than around the edges."

M "Hasn't any shape; can't say anything about its size."

Sk "Red predominated toward upper right hand corner, just around the border."

Texture.—By "It's filmy."

Gn "Like looking at grey through a mist. The mist is darker than the object." "As if you could color gas and have it so finely mixed that you couldn't see puffs in it." "A filmy appearance."

Mo "The color is filmy, airy, like a mist at night; foggy, not regular like light thrown from a lantern; a film." "The grey is more impenetrable than the color. The other is thinner than this. The grey is like a surface that I can see into."

Sk "The red is filmy; flamelike."

Disappearance.—By "Sometimes it is just a flash. Sometimes it stays a perceptible length of time." "When I first turned around it was almost normal, but it faded to a dark purple." "The fading takes some time, but cannot be measured in seconds. It gives the impression of fading

[18]The white paper used in this experiment, the Milton Bradley glazed white, was slightly orangish; hence the blue and green needed by Bi and Ho to match satisfactorily the Hering grey.

rather than of snapping, like pulling a string of an electric light quickly, so that the light goes off quickly, but you are aware of decreasing intensity."

M "It is gone as soon as I see it, as if it were fading away as I see it. I just see it going. Fading is like the vanishing of a mist, but the stuff is so vague that it can't really vanish, it fades."

Sk "Red goes. Going is not sudden but gradual; wave-like. Doesn't take a long time. Easy movement, nothing startling or sudden, still it doesn't lag."

According to their own reports, the instability of the color which By, M, and Sk saw when they first turned around accounted for the predominance of blue in their matches. According to these reports, in the fading of the color the red went out before the blue, so that the matches were for the color which had already changed from its original appearance toward blue.

By (reports given after his first match, which was made from the more stable color) "I got it fine when I first turned around. Absolutely certain of that patch of color. Could have matched it again, if I could only have got it consistently."

Mo "Just as I got around there was a mist just as pale as could be. The color was like the original, but I couldn't match it because the grey came in and made it darker. The mist is blue but looks pink. The pink wabbles and changes."

Sk "Red purple like standard. The red goes out and so does the blue, but the blue seems more stable. Can't match red because I can't find it when I look for it."

In this experiment, because of memory color, impressed through long familiarity, a jar, whose untransformed color was "tangerine," was seen by two *O*s as fuchsia and by three as the color of an "Indian pink"; and a jar whose untransformed color was grey was seen by two *O*s as fuchsia, by one as plum color, by two as lilac, and by two as peacock blue. All of these colors were objectively measured.

The spontaneous descriptions of the four *O*s, who noticed that the color which they saw did not cover the entire jar and was instable, indicate that, in its ultimate psychological nature, the memory color is a film.

IV. Conclusions

When we first began our task of giving to the transforming images, typified by the memory color, their systematic setting, we knew them only as the surface colors which Hering and Katz described and the tones and odors which Stumpf and Henning mentioned; and our only tools were the conditions of appearance stated or implied in the discussions and instances of Hering and Katz. We found, however, that the range of the transforming image, which we now called the imaginal overlay, was not limited to the surface colors, tones and odors of very familiar objects, but that it included the qualities in all sense modalities, attributes other than quality, and such perceptive characters of experienced objects as size, form, texture and position. Indeed, it became more and more apparent, as we proceeded, that the

imiginal overlay, in some form or other, plays a not unimportant part in most, if not all, of our everyday perceptions.[19] To learn just what this part was, just how well the overlay played it, and under what conditions it played it best, we adopted two courses: (1) we studied instances of the overlay as we found them occurring in every day life, and (2) we sought, under controlled conditions, to examine carefully the overlay itself. As we went on the conditions, hardly more than latent in the discussions of Hering and Katz, became continually clearer; and we were finally able to define them as: (1) the object-consciousness, (2) an uncritical or casual attitude toward the transformed character of the perception, and (3) familiarity with the perception.

The means of discovering our casual instances were three: (1) an accidental change in a perceptive experience, (2) a doubting of a perception which caused a change in its experience, and (3) a realization of the inadequacy of a perceptive experience to the meaning of the perception. It soon become evident that a mere cataloguing of these instances under rubrics representing their means of discovery was inadequate to telling all that could be known about them; and that to bring them into systematic relation several cross-classifications were necessary.

The first of these bore upon the immediacy of the familiarity (which need amount to nothing more than the lack of a feeling of strangeness) of the perception; the second, upon the relation between the meaning (or meanings) of a perception, the differences in the critical attitude, and the resulting transformation; and the third, upon the adequacy of the overlay itself. We could assign to its proper place in these three classifications every casual instance discovered by the three methods indicated above, and every introspective report given in the controlled experiments.

Our experimental search took us into two different paths; the first led to a study of the memory color transforming the five natural objects, grass, snow, coal, gold, and blood, which Hering and Katz had mentioned; the second to an investigation of the overlays in the perceptions of the various sense modalities. Because an attitude critical enough for equating their colors dispelled the overlay from the five natural objects, a quantitative measure of their memory colors was impossible;

[19] We may instance a discovery made by one of our *O*s in the course of this investigation. The *O* in question works with 'near' or 'reading' glasses. In the familiar surroundings of his study he is able to read across the room the titles of familiar books and to mark and respond to the changes of expression on the face of a familiar visitor—until the need of careful scrutiny arises: then he at once becomes aware that books and face are mere indistinguishable blurs, and automatically changes his glasses. The imaginal extension of 'near' vision was not noticed until the *O* was asked to look out for cases of imaginal overlay.

yet the results of the first five experiments proved that in the seeing of the familiar objects the presence of a memory color is common and that it plays a dual rôle. (1) If the visible area of the object is fairly large and under an observational attitude would be mottled, the memory color causes it to appear uniform and in the best color visible under the observational attitude. (2) When the visible area is small and, though uniform, would under the observational attitude be weak in chroma and altered from the normal in hue and tint, the memory color enhances the chroma and corrects the defects in the hue and tint of the entire area.

The investigation of the perceptions of the other sense modalities not only substantiated the fact, first disclosed by the casual instances, that in these perceptions there are images exactly analogous to the memory colors of the visual perceptions, but it also gave us better phenomenological descriptions of the totally and partially adequate overlays. Furthermore every experiment either extended or refined our knowledge of the conditions of the overlay's appearance. The smell experiment showed that the extremely critical attitude, the sceptical attitude, completely ousts the overlay. The tonal experiment indicated that even a modified form of the critical attitude, a careful attending for the overlay, is unfavorable to its appearance. Yet the very next experiment, that dealing with taste and smell, demonstrated that a strong feeling of familiarity can prevent this careful attending from completely (or even partly) dispelling an averlay. This experiment moreover proved conclusively that one impressive experience with an object is sufficient to make it familiar. The results of the experiment with lifted weights took a slightly different turn; they showed that, even though the three essential conditions are favorable, the overlay appears more readily when the difference between it and the untransformed experience is not too great. The results of the touch experiment verified the results of all preceding experiments, afforded more detailed descriptions of partially adequate overlays and their usual manner of disappearing or fading and made possible the construction of a chart which demonstrated that the closer the similarity between the required overlay and the untransformed experience the greater is the probability of the appearance of this overlay.

For a complete story of the overlay, only two things were now lacking: first, an objective record of a memory color, and secondly a psychological description of it. Our last experiment supplied both needs; it gave us quantitative measures of strikingly different overlays which transformed the same presented object, and revealed the fact that in its ultimate psychological constitution the memory color proper is a film.

THE PRINCIPLES AND TECHNIQUE OF MENTAL MEASUREMENT

By TRUMAN L. KELLEY, Stanford University

This contribution has been in part inspired by two articles written by E. G. Boring,[1] and would have appeared earlier had not difficulties in connection with the second part of my treatment caused delay. Boring's conclusions are generally destructive, and tend to leave one with the feeling that there is no sound statistical basis for mental measurement, and little for other psychological measurement. My treatment readily falls into two parts: a criticism of Boring's negations, and a specification of certain things which I consider to be essential in a sound statistical procedure as applied to mental measurement. I shall quote freely from Boring's articles, referring to the first as "Math." and the second as "Logic," but a reading of them entire in connection with this paper is urged.

PART I.

If two series of measures are homogeneous, *i.e.*, if they measure the same basic phenomena, there will of necessity be much in common between them. Thus if 1000 reaction times of a certain individual give a certain distribution, and if a second 1000 by the same individual are obtained, under conditions as nearly similar to the first as possible, we shall expect the frequencies in similar categories to be approximately the same in the two series, if they are truly homogeneous. If these frequencies are quite different we must conclude that, owing to practice, fatigue, or other unanticipated and unmeasured cause, the series are not homogeneous.

Pearson has defined the statistical treatment for making the comparison between two series such as these, each of which is influenced by random errors. The treatment follows that of the more elementary problem—the comparison of a series influenced by random errors with a standard invariable series—which likewise involves the basic question of the significance of a difference. We shall consider the simpler case. Let it be given that individual A makes the record indicated in the third line of the accompanying table, and let it be required to determine how significant is the difference between this record and the standard record given in the first line of the table.

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[1] Mathematical vs. scientific significance, *Psych. Bul.*, Oct., 1919. The logic of the normal law of error in mental measurement, this JOURNAL, Jan., 1920.

Reaction Times	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*	*j*	Sums
Given standard distribution	1	19	28	25	13	7	3	2	1	1	100
Obtained distribution of reaction times of individual A	0	12	28	30	14	4	5	4	1	2	100
Cell divergence	−1	−7	0	5	1	−3	2	2	0	1	0
Cell square divergence	1	49	0	25	1	9	4	4	0	1	
Cell square contingency	1/1	49/19	0/28	25/25	1/13	9/7	4/3	4/2	0/1	1/1	10.275

Square contingency = χ^2 = 10.275 No. of cells = 10

From Elderton's tables P = .330 when χ^2 = 10.275 and the number of categories is 10. Note that numerical values for the reaction times have not been needed. Thus *a*, *b*, *c*, etc. is not a quantitative series; it is not even an ordered series; all that we may definitely say of *a*, *b*, *c*, etc. is that they are distinct categories or cells, and in their entirety cover the whole range of reaction times. Of course, if the series is an ordered series, or if it is a quantitative series, the contingency treatment given above is in no way altered. The calculation of χ^2 is simple as shown, but its meaning is complex because its value depends upon two factors, the similarity between the observed and the standard series, and the number of cells. In order that the number of cells may not be a factor in the interpretation Pearson[2] has developed a "goodness of fit" method, involving "P" which is obtained from χ^2 and the number of cells.[3] The meaning of P is stated by Boring to be the "probability that the difference is not due to chance." In more detail we can say that P is the probability that a random sampling of N measures of the distribution represented by the standard series will yield as great a divergence from the standard series as that given by the obtained series.

What assumptions are involved in the derivation of the measure P? The answer to this question is important, as it accounts for much of the difference in point of view between Boring and myself. (See especially p. 418 of this article.) I can discover but one assumption: it is that, if the obtained series is in reality a random sampling of the series given by the standard, then the random distribution of each cell frequency around the standard cell frequency as mean is given by the normal law of error. To illustrate: the standard cell frequency for cell *d* is 25. It is assumed that successive random samplings of 100 measures, each of the same basic data as represented by the standard, will yield sometimes more and sometimes less than 25 in cell *d*, and that the distribution of the numbers yielded will give a normal distribution with 25 as its mean. There is at times an error in this assumption, which Pearson has recognized, and that is that, if the theoretical call frequency is very small, it is more reasonable to assume a skew binomial distribution than a normal distribution to represent the

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[2]On the criterion that a given system of deviations from the probable in the case of a correlated system of variables is such that it can be reasonably supposed to have arisen from random sampling, *Phil. Mag.*, July, 1900.

[3]W. Palin Elderton, Tables for testing the goodness of fit of theory to observation, *Biom.*, V., 1, 1902.

distribution of cell frequencies resulting from successive samplings. Pearson therefore objects to the use of this method, if such small theoretical cell frequencies cannot be avoided. As to just what constitutes a small theoretical cell frequency, in the sense that the error thereby introduced into P is material, that is not an issue raised by Boring. If raised it could be treated numerically, as the difference between skew binomials and normal distributions is well-known; and I venture to say that it would lead to substantially the same conclusions as reached by Pearson. This particular, and minor, shortcoming of the measure P is therefore not the point at issue. Boring does object to placing reliance in the measure P, though, as I shall show, he is not consistent in this in as much as he later recommends its use. I quote (*Math.*, p. 336) with the purpose of discovering the nature of Boring's objections.

"It is a common experience of scientific persons working with human data that these formulae frequently give values for the probability of differences that are 'too high.' One works, for example, with the performances of a group of women and a group of men in a mental test and one finds a 'significant' difference,—perhaps a probability that 99 times out of 100 the men will do better than the women,—and yet one is convinced that there is no 'truly significant' difference indicated. Or one determines the deviation of an observed curve from an ideal form and finds, let us say, that only 2 times in 100 would data that tend to follow the ideal form deviate as much from the ideal as do the observed data; and yet in plotting the observations along with the theoretical form he may note that the two functions are sensibly the same, and may feel inclined (if he is not scared off by Mr. Pearson) to say that the ideal function actually does represent his data. It is with the basis for this particular scientific attitude that I am concerned.

"It appears that the apparent inconsistency between scientific intuition and mathematical result is not due to the unreliability of professional opinion, but to the fact that scientific generalization is a broader question than mathematical description."

If we "feel inclined" to draw a conclusion at variance with the data, that settles the matter. In this paragraph quoted Boring reveals a conviction which permeates his article. For a conviction to precede an argument is good scientific procedure; but the merit of the conclusion depends upon the rectitude of the argument; and certainly before an argument is made it is unsound to say that "it appears . . . that scientific generalization is a broader question than mathematical description." Nothing of the sort "appears" at this stage, or in my opinion at a later stage, of the argument.

I would be willing to defend the proposition that "scientific quantitative generalization is not and cannot be broader or more exact than the mathematical statement." I have added, for exactness, the word "quantitative," and this word should have appeared in Boring's statement, as he is here specifically discussing a quantitative measure of probability. To quote further:[4] after a discussion of sampling Boring states (*Math.*, p. 337):

"So it happens that the competent scientist does the best he can in obtaining unselected samples, makes his observations, computes a differ-

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[4] *Math.*, p. 336, footnote: "The differences between Pearson on the one hand, and Merriman and Airy on the other, *Phil. Mag.*, *op. cit.*, 171 ff., are of this order; Pearson is statistician, and Merriman and Airy scientists. It is interesting to find Pearson shifting ground in *Biometrika*, 2, 1903, p. 367, from a statistical result that lengths of forearm do not fit the Gaussian curve to a scientific conclusion that they do." I cannot pass this over without objecting. A very careful reading of the p. 367 leads me to an opposite conclusion, and I commend the citation to the critical reader.

ence and its 'significance,' and then—absurd as it may seem—very often discards his mathematical result, because . . . "[5]

The procedure described is such as I believe a competent scientist never resorts to. If he will not trust such mathematical findings as are contrary to his wishes, that fact in no sense provides scientific warrant for discarding them. "Absurd" hardly characterizes this procedure. Is it not rather a matter of the honesty of the investigator? The intent of this quotation seems to me to be to give warrant for keeping and using data if they support an established conviction and otherwise discarding them. I do not believe that Boring would do or defend this, and I should be much interested in learning his present view. The preceding quotation does not stand alone, and the following scarcely improves the situation (*Math.*, p. 337):

"Since in the nature of the case it is impossible for him (the scientist) to state in numerical terms the degree of representativeness that his samples possess, conclusions must ultimately be left to the scientific intuition of the experimenter and his public. Such an outcome with respect to the measure of the probability of difference is not wholly satisfactory, but it is inescapable."

Have we returned to "intuition" as a method of proof? I think not; nor in fact do I think Boring has whole-heartedly done so; for it will be obvious from quotations that he himself later (see quotations on p. 417) advocates a mathematical means of proof.

I have quoted extensively from this early article and with a part of it I agree,—particularly with the last sentence: "The case is one of many where statistical ability, divorced from a scientific intimacy with the fundamental observations, leads nowhere." If Boring will interchange the words "statistical" and "scientific" I will still agree in this matter of quantitative scientific investigation.

In turning from this earlier article to the later we find the same distrust of certain, I fear none-too-well appreciated, statistical procedures. I am well aware that I do not appreciate as one should the subtle and important logical foundations of statistics, but that does not prevent my being concerned over the dangerously large mote in my fellow psychologist's eye. With reference to "the logic of the normal law of error in mental measurement" I contend(*a*) that Boring has looked upon the logic of probability as altogether deductive and has quite failed to realize its inductive bearing, and that as a consequence "probability" can no more seem adequate to him as a means of handling ordinary psychological problems than would any other purely deductive treatment; (*b*) that certain striking phrases such as "knowledge cannot be wrought out of ignorance" and "the equal distribution of ignorance" and "cogent grounds for assuming" have so appealed to Boring and laid hold of him that they have been used in improper senses and connections to the obscuration of the argument; (*c*) that Boring has made a very material mathematical error: in his claim that, in the sense presented, the psychometric function is a function of the unit of measurement; and also (*d*) in his failure to realize that the assumption of normality which he condemns is involved in the very methods which he advocates. As a consequence of the foregoing I contend that the limitations which Boring ascribes to much of statistical procedure are unwarranted and that the appropriate field of statistics is much broader than he pictures.

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[5] I have closed this quotation in the middle of a sentence, because so doing does not change the meaning with reference to the point I make, and because the entire sentence seems to me to be confusing. The rest of the sentence is ". . . in his judgment the mathematically 'significant' difference is nevertheless not large compared with what he believes is the discrepancy between his samples and the larger groups which they represent."

Before taking up these four points in order I must admit a difficulty in getting consistent meaning from all that Boring writes. For example, we find (*Logic*, p. 14):

"We remarked that Pearson in 1894 gave evidence of being influenced by the sanctity of the normal law. His faith was not great for he noted its limitations in 1895*. In 1909 the faith was gone.†"

And again (*Logic*, p. 25):

"This quotation is as near a confession of what the Biometric School is trying to do as we are likely to get. It is frankly seeking always to see Gauss in nature."

Either quotation taken separately would be understood as expressing Boring's belief; but taken together, which one should be at liberty to do, they produce a certain disquietude.[6] This peculiar unawareness of Boring's right hand of the commitments of his left cannot at times be so easily dismissed as in the preceding instance, as it is more material to the argument. This is important in connection with my first point (*a*).

(*a*) Boring quotes Arne Fisher (*Logic*, pp. 4 f.) to the effect:

"It thus appears that a rigorous application of the principle of cogent reason seems impossible. However, a compromise between this principle and that of the principle of insufficient reason may be effected by the following definition of equally possible cases, viz.: Equally possible cases are such cases in which we, after an exhaustive analysis of the physical laws underlying the structure of the complex of causes, influencing the special event, are led to assume that no particular case will occur in preference to any other."

Dr. Fisher is an authoritative exponent of the deductive school (with some exceptions and qualifications, the Continental school) of statistics. In line with this point of view we find Boring writing (p. 2):

"We can not say that *a* is twice as likely to happen as *b* because it happens twice as often, for this is simply to reason from frequency to probability *and ultimately to identify the two.*"

Also in line with it (*Logic*, p. 6):

> "Let us state the case in roughly syllogistic form.
>
> The tossing of a penny gives, when indefinitely repeated, 50% heads;
> The event in which we are interested is a tossing of a penny;
> The event in which we are interested gives, when indefinitely repeated, 50% heads.

This leads us to predict what will happen to our particular penny, if it is tossed repeatedly; and, what is more important, it defines for us the place of cogent reason and the sphere of ignorance. We must know what a penny does when tossed again and again, either directly from observation or indirectly by inference from other observations. The major premise must be based upon cogent reason and *there is no limit to the exactitude of the knowledge.* Ideally the major premise may be considered to be indubitably established. Moreover we must know that the event in which we are interested is the toss of a penny in the sense of that term in the major

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Philos. Trans., loc. cit.

†*Philos. Mag.*, 5th ser., 50, 1900, 173.

[6] I may mention my personal conviction, not based upon an actual count, that Pearson assumes normality in the majority of his developments, but, except where made a major and admittedly moot issue, it is an assumption of normality of errors of observation or of errors in frequencies in the case of random sampling, and not an assumption of normality of form of distribution of mental or biological trait.

premise. Our minor premise, by 'an exhaustive analysis of the physical laws underlying the structure of the complex of causes influencing the special event,' *must also be established and without limit of accuracy by cogent reason.* Of what then do we remain ignorant? Of the manner in which the particular, in the middle term, is subsumed under the general." Boring seems aware that this is not the only point of view which statisticians have taken, as I will immediately show by quotations, but this is the specific point of view which he criticises. Personally I cannot defend probability as having rigid, or rather universal, deductive warrant and I am glad that I cannot, as the method is much broader than this one logical procedure. Immediately preceding the previous quotation we find:

"In the initial instance this fact (that heads come as often as tails when tossing pennies) must be observed. Thereafter, we may reason that other homogeneous, one headed, well-tossed pennies will in the long run, come heads fifty per cent. of the time, and we may even go further and reason that a homogeneous, six-faced, well-tossed die will in the long run come up equally often on every face. What we do is to assume probability on the grounds of observed frequency and its conditions."

There is also given a footnote upon this page (*Logic*, p. 5) citing Edgworth, Pearson and Westergaard as approving the assumption of probability upon the grounds of observed frequencies. After admitting, as Boring here does, that we assume probability on the ground of observed frequency I see no consistency in maintaining the position represented by the italicized portions (italics mine) of the preceding quotations. To deny the right to identify frequency with probability (*Logic*, p. 2) and then to do so (*Logic*, pp. 5f.) would present serious difficulties in Boring's argument were it not for the fact that he seems immediately to forget the second (*Logic*, pp. 5f.) point of view.

This identification of frequency with probability necessarily enters into social statistics. It is the inductive element. It belongs in the process and is not, as Boring implies in the sentence (*Logic*, p. 8) "At this level common sense takes care of the theorist," an outside factor in the shape of "common sense" which comes to the rescue. I know of no single practical problem in insurance, enonomics, education, psychology or biology where earlier observed frequencies have not been taken as the guide to later expectation, and this is inherent in a sound statistical procedure. Even the Mendelians take an observed ratio of, let us say, 53:47 as suggestive of 50:50 and not of 75:25. The pure inductive treatment would be to take such a ratio as presumptive of 53:47, and to refine or correct the assumption or expectation as more evidence is gathered. This assumption is never "indubitably established" and it never can be, as exactly comparable situations at two different periods of time cannot be guaranteed. It is purely theoretical to consider such a situation. Part of the inexactness of the earlier discovered frequencies, as indicative of frequencies operating in general and for all time, is discovered by the later experiment, and thus a second and more accurate concept of the frequencies in the various categories is obtained. Thus is truth gradually wrought out of ignorance.

Ths syllogistic statement in the quotation given on a preceding page is not typical of the ordinary problem, which may be stated:

1. The sum total of our previous, though admittedly fallible experience, suggests that under certain conditions fact A occurs x per cent. of the time and fact B (100-x) per cent.

2. The conditions with which we have to deal seem to be the required ones.

3. In this present experiment fact A does occur $(x+\delta)$ per cent. of the times and B $(100-x-\delta)$ per cent., which, in the light of the size of the probable error of δ, suggests a revision, slight or great as the case may be, of the major or minor premise, or of both.

If Boring objects to this statement of the case I must reply that I cannot argue with reference to a situation where the "major premise is indubitably established", or to one in which the minor premise is "without limit of accuracy." Such a situation parallels no practical problem of statistics.

We remain as ignorant after the experiment as before "of the manner in which the particular, in the middle term, is subsumed under the general." To remove this particular kind of ignorance has not been the purpose of the experiment, which in general has been to increase the state of knowledge of the premises. When the first and second premises have become so well established that a new investigation suggests insignificant revisions of them, we have attained the certainty sought in our problem, whether it be in mental measurement or some other social field. I could cite numerous illustrations of this particular procedure in mental measurement studies, and cannot cite one of the type of Boring's syllogistic statement.

(*b*) The doctrine of the "equal distribution of ignorance" as it has grown out of the purely deductive concept of probability has, in the hands of some, led to such excesses as the determination of the number of elements in Sirius. I can no more defend this than I can Quetelet's (and I regret to say that he is not alone) assumption of normality of distribution to prove normality of distribution. There is, however, a meaning in which the expression is serviceable. Let us suppose (1) that all the information available (say a brief previous experiement or a number of casual observations) points to 50 per cent. of A's and 50 per cent. of B's, and (2) that as nearly as we can tell the present events are similar to the preceding; then (3) with reference to any single additional measure our ignorance is equally distributed, since we have no criterion as to whether the measure will be an A or a B. Under these conditions to assume a probability of 0.5 for A is not illogical, though of course the assumption may be wrong as subsequent experiment might show.

Under these conditions are there "cogent reasons" for the assumption? To "assume," according to Webster, is "to take for granted as the basis of argument." Thus an assumption is an hypothesis or proposition to be accepted tentatively and tested later. An hypothesis is a guess, and fundamentally an assumption is also. "Cogent" means "powerful," "strong," and as used by Boring and further defined by him, "there is no limit to the exactitude of the knowledge." Thus, in substance, *we have reasons for our guess so powerful that there is no gainsaying them.* The very statement reveals the inconsistency, for an assumption so fortified is no longer an assumption but a certitude. Boring has so defined "cogent" that there is no place left for "assumption," and that is exactly the point he makes; that there should be no "assumption." This position is untenable, as every act of life testifies. The bases of conduct are probabilities, not certainties, and the scientific procedure is to measure and express these probabilities, continually assuming the more and more probable as the basis for future expectation. Cogent "without limit to the exactitude of the knowledge" is without a parallel in reality, and to get any meaning at all I must discuss degrees of cogency and degrees in the reliability of assumptions. In every case the probable error in the assumption increases as the cogency decreases, and *vice versa.* But a little thought will convince one that there is an assumption-aspect in every act of voluntary (including experimental) life, and a theory of statistics that did not parallel this would be useless. The real problem, then, is ever to decrease the violence of the assumption, and secondly, when either one of two assumptions will serve a given situation, to choose the more probable. Fortunately statistical method does not depend for its warrant upon such cogency as Boring advocates, and an assumption based upon the best obtainable evaluation of preceding experience is a sufficient starting-point in the process of increasing

knowledge at the expense of ignorance. Thus, to answer the question put at the beginning of this paragraph, I should say the reasons were sufficient for the purpose.

(*c*) If the distribution of the heights of a number of individuals, expressed in inches, gives a normal curve with mean M_1, and standard deviation σ_1, we shall still have a normal distribution if the heights are expressed in centimeters, though the mean M_c and the standard devation σ_c will have numerical values which are different from M_1 and σ_1. Again, if we measure the heights from a table top three feet above the floor upon which the individuals stand we should obtain measures giving a normal distribution. Thus any linear transformation of the scale does not change the form of distribution. This is fortunate for, if a biological unit (possibly the diameter of some elemental cell) exists, it is only necessary that the scale of units used be related to it by an equation of the sort

$$y = a + bx$$

in which a and b are constants, x the number of biological units, and y the number of units in the scale used, in order that the form of distribution shall be the same whether the biological or the other units are used. It may be beyond likelihood to ascertain the biological unit and measure upon a scale of such units, though quite possible to determine a scale linearly related to it. This relationship between units holds whether the form of distribution be normal or otherwise; thus a Pearson Type III curve remains a Type III curve if another scale linearly related to the first is employed.

Boring's reference to the "all important matter of the unit" does not make it clear that the very great freedom in units here indicated is permissible. In fact Boring seems to recognize this fact in one part of his paper and to deny it in a second. He states (*Logic*, p. 15): "Suppose then that nature does conform to the normal law; we shall not in practice be fortunate enough to obtain normal curves of distribution unless we have chanced to use nature's own unit of measurement, or some unit directly proportional to it."

The statement "directly proportional" implies an equation of the sort

$$y = bx;$$

whereas the more general relation

$$y = a + bx$$

is sufficient. However, even the freedom permitted by the use of proportional units is brought into question on p. 23 and, as I read it, actually denied on p. 27. We find (*Logic*, p. 23):

"When we change from stature to the forearm we have no guarantee that the inch means the same thing biologically. And we must use a biological unit, if we are to bind our class together by a biological concept, if we are to predicate the normal law of all *human* characters. That the form of distribution is a function of the unit is unescapable." This last sentence (strengthened by the first) seems to me to be an assertion that the particular unit used must be identically the biologic unit if we are to obtain a concept as to real biologic form of distribution. Again (*Logic*, p. 27):

"To say that the phi-function of gamma is the psychometric function is equivalent to saying that the dispositional variations of the psychophysical organism, when measured in a scale of units proportional to the scale in which the stimulus is measured, follow the normal law. If the lifted weights are measured in grams and the psychometric function approximates the phi-function of gamma, then the organism varies in the amount of its disposition for judging 'heavier' or 'lighter' according to normal law, provided always that the amount of this disposition is measured in grams. This

relationship is scarcely obvious and the reader may need to read elsewhere before he can accept it."[7]

The first sentence in this quotation is none too clear to me, but it seems irreconcilable with the second. But let us continue. In a footnote on the same page we read:

"Keller used Wundt's fall phonometer, and the all-important unit of stimulus, which is only just mentioned, is the cm. The psychometric functions of the lifted weights are for grams."

Here again are not the words, "provided always that the amount of this disposition is measured in grams" a positive assertion that one may not use ounces, pounds or kilograms? Is not "the all-important unit of stimulus is the cm." an assertion that inches would not have served? I cannot understand why Boring should make these statements unless he thinks that the situation here, psychometric functions, is upon a different basis from the earlier situation, frequency distributions. In dealing with distributions he surely knew that a proportional unit of measurement would suffice. As further evidence that he was aware of this point I would mention that his own illustrations of changes in units which change the form of distribution or change the form of the psychometric function are all non-linear transformations, being in fact of the type

$$y = x^2$$
$$y = log\ x$$
$$y = sin^2\ (x/2).$$

Again, on p. 28 we find:

"In the case of the 'introspective' psychophysics we are not to expect a generalization, because everything depends on the unit."

This, as it stands, and doubly so in connection with the preceding quotations, conveys a very wrong impression. Whereas it is quite beyond our present prospects to isolate a particular biological mental unit, it is rather more conceivable that, in fields where there are such natural units that play a part in life, they will manifest themselves as some simple function than otherwise. If this function is linear, our measures are immediately serviceable; if logarithmic, one simple intermediate step is needed, etc. If there is such a thing as a biological mental unit it is difficult for me to conceive that our sensed awareness of it is not related to it in some very orderly manner, whereas I judge it would seem to Boring difficult to conceive that it could be so related. I must, however, qualify this statement, for Boring does seem to realize that there is something elemental in the "sense-distance." He writes, p. 31:

"The great difficulty is, as we have just pointed out, to find anything that we may properly call a psychological unit. The sense-distance is such a unit. It is a unit of measurement that is mental *per se*;" and, footnote:

"The thing is so simple when it is seen!"

Science has apparently succeeded in determining units linearly related to natural units in the matters of length, mass, time, temperature and in several other chemical and electrical fields, and all of these are available to the psychologist as starting points. Errors in human judgments in these fields and measured in these units have been found to distribute themselves according to the normal law (or in distributions so close to the normal that there is no present value in distinguishing between them). I am setting up no fetish in pointing out this truth. As a consequence of this situation I claim that the assumption of normality of distribution of errors

[7] Boring's footnote is: "Boring, A Chart of the Psychometric Function, *Amer. J. Psych.* 28, 1917, 465 ff." I am unable to follow Boring's reasoning here as I find nothing in this very lucid article bearing upon the present point.

in human judgments in other fields is the most reasonable one to make as a point of departure. I shall return to this point later, but would first examine into Boring's affection for the normal curve.

(*d*) I could quote at length and entertainingly, showing that Boring considers the idea of normality to be a fetish and the assumption of it under any circumstances, "with the bare hint of an exception . . . in the psychometric functions of errors of observation," unwarranted. This one possible exception will not concern us in this present section (*d*).

Now what is left in the way of statistical devices? I quote from p. 32, and secondly from p. 32-33.

"We are left then with the rank-orders of our psychological quantities, given by reference to a fixed but arbitrary extra-psychological scale; and it is with these rank-orders that we must deal."

"There is nothing new in the contention that mental measurement is impossible, whereas now we do gain the assurance that rank-orders at least are validly demonstrable. And there is a great deal that can be done with rank-orders. We can deal with frequencies, medians and quartiles. For example, it is considerable to know of two groups that the lower quartile of the ranks in one overlaps and is practically coincident with the upper quartile of the other. What we must remember, however, is that we are dealing with the statistics of medians, quartiles, contingencies, and correlation ratios; not with the statistics of averages, standard deviations, coefficients of correlation, and linear regressions. All those statistical constants, that imply a scale of equivalent units, violate in use the conditions of the case and lead to a precision of result that is an artifact."

Let us examine these permissible statistical procedures. A correlation ratio is the ratio of two standard deviations, each of a variable which is quantitatively measured, *i.e.*, measured upon a scale of equivalent units. Surely Boring did not intend to approve of this. We may put this down as a mathematical slip, and proceed to the other measures. For the calculation of a median or quartile in the case of grouped data some assumption as to the quantitative relations between groups and the distribution of measures within groups is necessary to avoid a grouping error. Since we must not deal with graded measures, we will assume that Boring has in mind such a large number of categories in his ordered series that there is no correction for grouping needed. Limitation to series with such large number of categories would in itself be a very serious practical handicap, but we will pass this over, and suppose that we have determined a median for a certain ordered series. Standing alone, it is meaningless. It is only in its relation to some other percentile of the same group, or of a new group, that it has significance. The comparison cannot be of numerical values, for quantitative measures are ruled out. We can therefore only make a comparison of the type 25 percent. of the scores of group A exceed 50 percent. of those of group B. But what of this fact? It is empty in meaning unless we can interpret it in terms of the magnitude of the difference between the two groups, *e.g.*, as .6745 σ, or in terms of the improbability of the situation arising as a matter of chance. The first means of interpretation is obviously ruled out; and the second, involving as it does a measure P, derived from frequency values of categories,—just as at the beginning of this article P was derived from χ^2 and the number of cells,—has involved in it the assumption of normality of frequency of distribution under random sampling, and is thus likewise ruled out. We cannot interpret quartiles or medians without resort to assumption of normality or some other form of distribution or resort to quantitative concepts. We have still to consider measures of contingency; χ^2 the square contingency, Φ^2 the mean square contingency, C the coefficient of contingency, and P the measure of probability already described. I will not weary the reader with a long exposition, but will simply say that the meanings of the first three of these are involved, and

that I at least am not able to interpret them without resort to one or more assumptions as to form of distribution. The meaning of P and the assumption of normality involved in it have been made clear.

Our conclusion therefore is that none of the measures advocated by Boring is free of those things which he inveighs against. It is this situation in particular which leads me to suggest that Boring's criticisms were made without due appreciation of the foundations of statistics. If he had realized that the assumption of form of distribution or of comparability of units is basic in every statistical treatment, I believe it would have led him to a discussion of the relative merits of different assumptions and not to a total condemnation of assumption. The dictum "assume not at all" is just as impossible in practical statistics as it would be in everyday matters of conduct.

I will quote from Boring's last paragraph, p. 33.

"The initial error in the application of the theory of probabilities was the assumption of the law of insufficient reason. It was wrongly supposed that knowledge could somehow be wrought out of ignorance. This very error, however, has never been routed. . . . But, if in psychology we must deal—and it seems we must—with abilities, capacities, dispositions and tendencies, the nature of which we can not accurately define, then it is senseless to seek in the logical process of mathematical elaboration a psychologically significant precision that was not present in the psychological setting of the problem."

To do what Boring considers "senseless" is to my mind the exact purpose of mathematical elaboration; and as the error of believing that knowledge can be wrought out of ignorance is still strong within me, I shall devote the rest of this article to a discussion of mathematical procedures, coupled with appropriate assumptions, which I anticipate will lead to an increase of knowledge.

Part 2.

It might seem axiomatic that there can not be a science of quantitative measurement until and unless there is established a particular unit of measurement. This is, however, true only in a limited sense; for it is quite conceiveable that one could have a science of physical phenomena in which the units were such that the scale of time intervals was the square of the present intervals measured in seconds, and in which the length scale was logarithmic as compared with the present scale in centimeters, etc. Of course, in terms of these new units, all the laws of physics would be stated by means of formulas different from and in general more cumbersome than our present formulas; but nevertheless we could have an exact science. The existence of the science does not lie in the units employed, but in the relationships which are established as following after the choice of the units.

A parallel situation holds with reference to mental measurement; so that, starting with units however defined, if we can establish important relationships between phenomena measured in these units, we have proceeded scientifically. The choice of the unit is purely a question of utility. It is preferable so to define it as to lead to the simplest possible algebraic statements of the important relationships. There may be many such

relationships, and the simplest units for one purpose may not be the simplest for a second. However, just as Euclidean geometry is more serviceable than non-Euclidean for all-around practical needs, so we may expect that there is one way of defining mental units which will be generally more serviceable than any other. Without denying the possibility of other workable systems, it seems that, in a civilization such as ours, steeped in the elementary associative and commutative principles of arithmetic and algebra, much is to be gained in simplicity and accuracy of interpretation if the units employed in mental measurement obey these well-known laws.

Let us view this from the standpoint of certain everyday problems. I shall use the terms "general intelligence" and "mental age" with their usual meaning, which is a sufficient definition for my present purpose. Here in the United States we have an elementary public-school system adapted to the progress of a child from grade to grade by yearly steps, and we frequently wish to know how far ahead or behind a child is in terms of time. Thus, "one year advanced" is an immediately serviceable concept for elementary school needs. For this reason the mental age and the mental year, or unit, in the mental age scale, are concepts which in spite of their shortcomings may be expected to persist. They are of uncertain value in the high-school, and very confusing in connection with still higher ages. All told they are not generally serviceable measures, and for three reasons closely related to each other: first, the mental year is such a unit as does not conform to the elementary laws of algebra; secondly (this is not yet indubitably established), disparate mental factors are thrown together; and thirdly, and possibly merely as a consequence of the first two, regression relationships when wide ranges in mental ages are involved are generally non-linear.

As to the first: if we add the mental year 5.0–6.0 to the basic mental age 5.0 we shall obtain a mental age 6.0; but were we to add the mental year 18.0–19.0, which is known to be practically zero, to the same basic mental age, we should obtain 5.0+ as the answer. Some may object, saying that we cannot make and are not called upon to make this second addition, but can properly make the first. This point is well made; and because it is, the peculiar non-additive character of the mental year is not directly apparent. It however does enter, indirectly, whenever calculations covering several mental years and utilizing means, standard deviations, product moment correlations, etc., are made. Otis[8] has pointed out several of the

[8] A. S. Otis, Some logical aspects of the Binet Scale, *Psych. Rev.*, 23, 1916.

many advantages resulting from a use of additive units, and I shall not dwell further upon this point.

Secondly: one can argue that the entire general intelligence, and therefore as a consequence mental age, idea is wrong, and that adding the ability to "copy a square" to "repeat six digits" and obtaining the "difference between a king and a president" is like adding a box of soap to a keg of nails and obtaining a bolt of cloth. There is enough experimental justification for this point of view that the mental measurer should ever be on the watch to discover qualitative differences which may be embodied in his seemingly quantitative results; but on the other hand the rich reward during the last half-generation in the more fruitful understanding of important individual differences, and the better adaptation of teaching method to individual ability which has been consequent to general intelligence measurement, fully justify the thorough examination of differences which are apparently qualitative with the end in view of discovering quantitative relationships. Whereas the second concept, soap + nails = cloth, has served no end, the first has led to procedures the general excellence and justice of which as compared with methods displaced can not be gainsaid. The quantitative treatment of mental differences needs no further justification than this, though its most ardent advocates will admit it needs much refinement.

Thirdly: when mental age is one variable and when any one of a few score of our psychological tests is the other, and when wide ranges of talent are involved, it is common to find a tendency of the regression line of score upon mental age to become parallel to the mental age axis at the upper end. It seems reasonable to expect that this result is mainly attributable to the lessened intrinsic significance of a mental year high in the scale as compared with one low in the scale. Thus, if the mental year is given progressively less numerical value as the scale is ascended, regression relationships will generally tend to become linear.

This same result could be accomplished by progressively changing the value of the units of the psychological test, the mental age units being left unchanged. This would, however, be an unfortunate means of bringing about linear relationships, because the units now employed in the better psychological tests, *e.g.*, National Intelligence, Army Alpha, Terman Group, Otis Group, Trabue Completion, Thorndike Handwriting Scale, Hillegas Composition Scale, etc., are very approximately proportional to sensed differences in ability as determined by opinions of competent judges. There is a warrant in the sensed difference which is basic, since the purpose in measuring and classifying by means of tests is to do better and more economic-

ally that which human judgments already attempt to do. The determination of individual differences in units, however divinely given, which have no discoverable relationship to individual differences as sensed by teachers and employers, or by pure psychologists if they are to interpret the test results, is without the specific value which we are regularly seeking. I find myself in hearty agreement with Boring upon this point; and though we might not define "sensed difference" in the same way I do believe our definitions would result in proportionate units, which is all that is necessary. Because the sensed difference is a mental unit per se, it is serviceable in handling problems which are mental per se.

I have endeavored to show that the mental year and the mental age have fundamental defects for quantitative statement and interpretation. The intelligence quotient (I.Q. = mental age ÷ chronological age) suffers from the defects already pointed out of its parents, mental age and chronological age, though to a certain extent these defects are recessive, not appearing in the offspring, as is apparent from considering the entirely straightforward way in which an I.Q. of 1.00 may always be interpreted. If the I.Q. is not greatly different from 1.00, and particularly if elementary school ages only are involved, it is probably a good measure, though not one the use of which we can defend for the most refined and theoretical work.

Let us consider in more detail what are the defensible bases for determining the units of a mental scale. The statement as made presupposes that we have an ordered series. I anticipate that much greater difficulty will be experienced in determining a homogeneous ordered series than in scaling it afterward. I hope at a later date to offer criteria for determining if a number of mental tasks involve one or more qualitatively different mental functions; but for the present discussion I shall assume that we are possessed of an ordered series qualitatively homogeneous, and wish to determine quantitative values for the various items in the series. The reader may have in mind a series a, b, c, . . . of brightness intensities, concerning which we know that they constitute a single series such that, in intensity, $a < b < c <$ etc. Making no use of physical measurements of candle power, strength of electric current, or the like, we wish to assign numerical values to the items in the series such that of items scaled 10, 11, 15, 16 the feeling or appreciation of difference (11–10) is equal to that (16–15), etc., throughout the entire scale.

The question arises who is to do the "feeling." To meet practical needs, it should be the type of person or persons called upon to make, or known to be capable of making, such judgments in daily life and whose procedures are to be assisted by

the possession of the scale, *e.g.*, painters, photographers, dyers. Scalings built upon the judgments of individual X might conceivably be different, and not merely different in some constant proportion, from the scalings yielded by the judgments of individual Y. If our purpose is not to study the idiosyncrasies of X and Y, but to obtain a scale of general utility, we should utilize many judges, giving such weights to their separate determinations as we may have auxiliary evidence to warrant.

There are different psychophysical methods for obtaining judgments of difference, and without attempting to designate some one as best we may state two axioms. (1) Since it is assumed that the scale constructed under psychological conditions A will be serviceable under practical conditions B, therefore the more similar A and B the less violent the assumption. Corollary: generally the greatest practical need for assistance is in the matter of fine judgments; therefore the "just noticeable difference" may well be incorporated in the derivation. (2) That method is preferable which shows the greater consistency with itself, *i.e.*, which results in scalings having, for a given number of judgments, the smallest probable errors. To profit by this axiom one would ordinarily have to resort to brief preliminary scalings by the various methods and roughly determine the probable errors.

Whatever method is chosen, some preliminary assumption as to the distribution of judgments must be made; and *the better this assumption is as judged by the distribution of judgments upon the completed scale the more accurate will the scalings be.* Not having seen this criterion of the validity of the assumption proposed or recognized by any previous psychological or educational scale-maker, I will give the steps in detail together with a numerical illustration. Consider the scaling of brightness intensities from the judgments of a single judge.

Lemma: A true, or biological, intensity scale for this judge exists. Upon it intensity items $a, b, c, \ldots$ have numerical values $A, B, C, \ldots$ A single form of distribution of errors of judgment for this judge exists (if this is not true the problem is much more difficult, though not necessarily insoluble). If by good luck we assume the true form of distribution and know the number of times each item $a, b, c, \ldots$ is judged more intense than each other item in the same series, we can deduce a set of values $A', B', C', \ldots$ linearly related to $A, B, C, \ldots$ Finally, we may calculate the form of distribution of errors of judgment of this judge in terms of the scale $A', B', C', \ldots$ and this form of distribution will be the true form. All of this will be obvious to the person who has constructed a scale, such for example as the Hillegas Composition scale.

Proposition: If the form assumed is not the true form, the final form calculated in the units of the A', B', C', . . . scale will not be the assumed form nor the true form but intermediate. Corollary: starting anew, assuming this intermediate form, second intermediate form may be obtained which is still closer to the true form. Etc., for still closer approximations.

I need not dwell upon the Lemma; but as the Proposition itself is far from obvious I shall cite three things in its support: (1) a brief handwriting scaling experiment; (2) a numerical illustration of the process; and (3) the reason for it as consequential to the properties of an average.

(1) Using rankings of judges (equivalent to assuming a rectangular distribution of values of handwriting samples) at their face value, I obtained scale values of the samples of handwriting and found that the distribution of samples in these units was approximately normal. Thus the assumption of a rectangular distribution did not lead to a final form of distribution which was rectangular.

(2) The following illustration is made brief and does not involve the assumption of normality. This fact makes it quite artificial, but it should be the more convincing to one who fears the surreptitious introduction of the "normal fetish." Let us then be given intensities a, b, c, d whose true values, measuring from intensity a are 0, 1, 3, 4 respectively. Further, the true distribution of errors of judgment of judge X is triangular, being represented by an isosceles triangle with base 10 and altitude 0.2. The equations of the lines defining this triangle are

$$y = 0;\ y = .2 - .04x;\ y = .2 + .04x.$$

Integrating this distribution we obtain the psychometric function. We shall need it for one-half the curve only. Integrating, $y = 2 - .04x$ we obtain,

$$\int y\,dx = .2x - .02x^2 \qquad (x \text{ varies from } 0 \text{ to } 5)$$

Thus the psychometric function, P, for the upper half of the curve is given by

$$P = .5 + .2x - .02x.^2$$

To illustrate: intensity b is one unit above a; therefore, substituting 1 for x we obtain .68, the proportion of times that b is judged more intense than a. Similarly, with this situation as described, the judgments will be as indicated in the accompanying table.

TABLE I.

	Proportion of "greater judgments"
$a > a$	.50
$b > a$	.68
$c > a$	.92
$d > a$	.98
$b > b$	.50
$c > b$	.82
$d > b$	.92
$c > c$	.50
$d > c$	.68

The problem before us is, knowing only the proportions of greater judgments as given in the table, to determine the function

$$P = .5 + .2cx - .02(cx).^2$$

The c has been introduced because all we need are units proportional to the true units.

We may first and incidentally note that the Lemma holds true here. Thus, let us assume a triangular form of distribution represented by the equations

$$y = 0;\ y = A - A^2x;\ y = A + A^2x$$

which is the general statement under the restrictions that the total area shall equal 1.00 and that the distribution be symmetrical. We then have

$$P = .5 + Ax - A^2x^2/2.$$

Being given proportions, P, and utilizing this integral of the distribution, we obtain x-values as follows:

TABLE II.

$a > a$	$.50 (= P)$	*therefore*	$x = .0$	(The solution $x = 2/A$ is clearly extraneous)
$b > a$	.68		$x = .2/A$	
$c > a$	.92		$x = .6/A$	
$d > a$	.98		$x = .8/A$	
$b > b$	.50		$x = .0$	
$c > b$	.82		$x = .4/A$	
$d > b$	.92		$x = .6/A$	
$c > c$	.50		$x = .0$	
$d > c$	.68		$x = .2/A$	

TABLE III.

Distance	$\overline{ab}$	$= (b-a) = .2/A$
	$\overline{ab}$	$= (c-a)-(c-b) = (.6-.4)/A = .2/A$
	$\overline{ab}$	$= (d-a)-(d-b) = (.8-.6)/A = .2/A$
	$\overline{ab}$	$= (d-a)-(d-c)-(c-b)(=.8-.2-.4)/A=.2/A$
Average	$\overline{ab}$	$= .2/A$

Similarly,	$\overline{bc}$	$= (b-c) = .4/A$
	$\overline{bc}$	$= (c-a)-(b-a) = (.6-.2)/A = .4/A$
	$\overline{bc}$	$= (d-b)-(d-c) = (.6-.2)/A = .4/A$
	$\overline{bc}$	$= (d-a)-(b-a)-(d-c)=(.8-.2-.2)/=.4/A$
Average	$\overline{bc}$	$= .4/A$

Similarly, Average $\overline{cd} = .2/A$.

Thus, assuming the true distribution, we obtain scalings by the method shown, in which each distance is the average of several separate ways of calculation, which bear the true proportion to each other. Now let us reverse the process,—let us start with these obtained scalings and the proportion of "greater judgments" corresponding, and derive the psychometric function P′. Beginning with some point, let us say a, we have the following table:

TABLE IV.

Proportion of "greater judgments"	at distance indicated above a
.50	.0
.68	.2/A
.92	.6/A
.98	.8/A

Attempting to fit a third degree parabola to these points we find that the cubic term drops out, and we obtain as the result

$$P' = .5 + Ax - A^2x^2/2.$$

The fact that this has turned out to be the equation assumed (the assumed distribution being of the form of the true distribution) is evidence that the Lemma holds.

Returning to the main development: we are to start with the data of Table I.; assume some incorrect form of distribution; under this assumption scale the intensities a, b, c, d; on the basis of these scalings calculate the form of distribution of judgments; and, finally, note if this is closer to the true form than the assumed form.

I shall assume a rectangular form of distribution (this certainly differs from the true triangular form). The equation is,

$$y = A$$

and the limits in x are, $-.5/A$ and $.5/A$

$$\int y dx = Ax.$$

For the upper half of the curve

$$P = .5 + Ax \quad (x \text{ varies from } 0 \text{ to } .5/A)$$

Being given the proportions, P, of Table I. and utilizing this integral of the assumed distribution of judgments, we obtain x-values as follows:

TABLE V.
(Compare with Table II.)

$a > a$	.50	*therefore* $x = .00$
$b > a$	.68	$x = .18/A$
$c > a$	.92	$x = .42/A$
$d > a$	.98	$x = .48/A$
$b > b$	.50	$x = .00$
$c > b$	.82	$x = .32/A$
$d > b$	.92	$x = .42/A$
$c > c$	.50	$x = .00$
$d > c$	.68	$x = .18/A$

TABLE V.
(calculated as were the distances in Table III.)

$$\text{Average } \overline{ab} = .08/A$$
$$\text{Average } \overline{bc} = .23/A$$
$$\text{Average } \overline{cd} = .08/A$$
$$\text{Sum } \overline{ad} = .39/A$$

In order to facilitate comparison of these values with the true values, let us set $.39/A = 4$, the true value for the distance *ad*. Then $A = .0975$ and we have, Table VI:

TABLE VI.

P	Value obtained under rectangular distribution hypothesis	True values
.50	$\overline{aa} = .00$	.00
.68	$\overline{ab} = .82$	1.00
.92	$\overline{ac} = 3.18$	3.00
.98	$\overline{ad} = 4.00$	4.00

Passing a cubical parabola through these four points yields

$$P'' = .50 + .268\,x - .065\,x^2 + .0070\,x^3$$
$$P = .50 + .20\,x - .02\,x^3$$
$$P' = .50 + .12\,x$$

The equations for P (the true function) and P′ (the assumed function) have been written down for comparison, the arbitrary constants being so chosen that the equations agree for the points (P = .50, x = .00) and (P = .98, x = 4.00). Since P″ is closer, in the constants involved in it, to P than is P′, the proposition holds for this case.

If we now repeat the process, assume the function P″, and determine a new psychometric function, we obtain

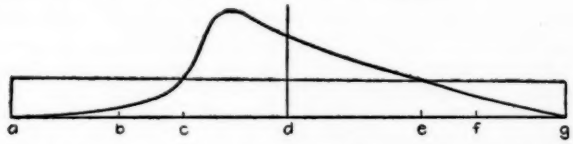
$$P''' = .50 + .207\,x - .024\,x^2 + .00064\,x^3$$

which is still more similar to the function P. In fact, by this time all resemblance to the original erroneous rectangular hypothesis is lost.

In actual work the procedure would not be as simple as in this articicial problem because, having but four points and assuming my data to be entirely trustworthy, I have not resorted to least-square methods for determining P″ and P‴. The principle, however, is the same in the more difficult problem, so let us examine the steps taken with a view to ascertaining what it is that leads to this fortunate result.

(3) I shall present the point of this section by considering the relationships between one form of distribution, the true form, and a second, the hypothetical or assumed form. For simplicity of treatment it will be necessary to deal with actual forms, but the reader will see that the treatment is entirely general and that the argument may be carried through in a manner similar to that here followed and to the same conclusion if he advises with his own special deity in determining the true form and is advised by his demon in selecting a hypothetical form. We will start with a true distribution, skewed, unimodal, and having diminishing frequencies as we approach the lower, a, or upper, g, limit of the range. For the hypothetical distribution we will take a rectangle upon the base $\overline{ag}$. Both distributions have total areas 1. The rectangle cuts the true distribution in points, measured along the base line, c and e. We will call the area upon base $\overline{ac}$ and under the true curve AC, and that under the hypothetical A′C′, and similarly for areas resting upon other segments. Note that AC $<$ A′C′; CE $>$ C′E′; and EG $<$ E′ G′.

Let us further say that point d on the base line is the median of each distribution. Then $AD = A'D' = .50$. Let us divide the $\overline{ac}$ and $\overline{eg}$ segments still further, in any arbitrary manner, obtaining points on the base line as indicated below and areas under the true and hypothetical curves as designated.



Areas-true	AB	BC	CD	DE	EF	FG
" -hypoth	A'B'	B'C'	C'D'	D'E'	E'F'	F'G'
	AB<A'B'	BC<B'C'	CD>C'D'	DE>D'E'	EF<E'F'	FG<F'G'

We will start scaling from point d. Thus if AE proportion of judgments designate stimulus y as greater than stimulus x its correct distance above x is $\overline{de}$. However, with the incorrect distribution as hypothesis it would require $A'E'$ proportion of "greater" judgments to result in the y being designated as just $\overline{de}$ above x. Since $AD = A'D'$ and $DE > D'E'$; therefore $AE > A'E'$, and a proportion AE of "greater" judgments results, under the incorrect form of distribution hypothesis, in a too large numerical value, let us call it $\overline{de} + \Delta$, for the distance that y is above x. Similarly, any proportion of "greater" judgments lying between AE and AG will result, under the incorrect hypothesis, in scalings numerically too large, but *decreasingly too large*; for the proportion AG results in the scaling $\overline{dg}$ under both the true and the incorrect hypotheses. Thus, if stimulus z yields a proportion AF of "greater" judgments than x, the scaled distance under the incorrect hypothesis will be $df + \Delta - \delta$ where $(\Delta - \delta)$ is positive. Subtracting the first from the second of these two scalings, we obtain for the distance apart of y and z the quantity

$$(df + \Delta - \delta) - (de + \Delta) = ef - \delta$$

which is numerically smaller than the truth. If we compare z with y directly, we are utilizing the distribution in the neighborhood of d and, as already shown, our obtained scaling for this neighborhood will be numerically too large; let us call it $ef + \delta'$. Thus we have established the proposition that the error in scaling, when utilizing the middle portion of the assumed distribution, is of the opposite sign to that present when utilizing outlying portions of the same distribution. As a consequence of this, in the long run, average scalings based on those from both

inlying and outlying portions of an assumed distribution diverge from the errors of either of these separately in the direction of true scalings. Thus, in the numerical illustration given, the averaging of four different ways of calculating each distance was the step responsible for the excellence of the scalings, and as a consequence for the obtained distribution being closer to the true distribution than was the assumed distribution.

We may now continue the argument of page 423, broken into by the proof of the Proposition used. The reader will clearly see that the preliminary assumption may be made strictly tentative and need not prejudice the form of distribution as finally determined. As Boring has properly stressed, if we have either the form of distribution or the unit, we can determine the other. Therefore, if we can secure a rank order of items in a homogeneous series, and if a single form of distribution of judgments (of the same judge for separate parts of the scale, or of several judges for the same part, or of several judges for several parts) exists, we can determine it and give numerical values to the items in the series. These numerical values will be additive in the ordinary sense, a difference of a unit in one part of the scale equalling a difference of a unit in a second part, as this has all been involved in the method of derivation,—the units being, of course, in terms of sensed differences. I believe that the sensed difference is the most serviceable and defensible basis for scaling a single homogeneous ordered mental series; but there are other very defensible bases resulting in scalings the units of which are equal in a very important sense, but not necessarily in the meaning of being sensed as equal.

We may start with the hypothesis that the variability of performance of several individuals, working at different times upon equally difficult tasks, under as similar conditions as can be established, is of a certain constant form; scale the difficulties of tasks on this assumption; calculate the form of distribution of variability of performance; and, if this form is the same as the assumed form, accept the scalings as final; but if not the same, revise the assumption and repeat the scaling as indicated in the case of judgments, attaining the true form of variability of performance; finally securing true scalings in the sense that the units are equal in terms of the variability of performance of individuals. It is very possible that the units thus defined would be sensed as equal, but we have no compelling a priori ground for asserting this. This method may prove cumbersome in actual use because of the necessity for certain controls,—allowance for fatigue, training, change of interest, inequality of assignments, etc.

A third method of scaling is to assume that the variability of a group is constant, assume a form of distribution, correct the

assumption as may be necessary, and scale the items of the series. Units here are equal in terms of the variability of the group; but, as we cannot hope to secure biologically pure strains when working with human material, we cannot expect to define our group, and accordingly our unit, in a manner that has ultimate significance. This, however, is a very theoretical criticism; for such a group as "white native-born 10-year-olds in the public schools" is quite definite enough to serve as a basis for most of our school needs. The method which has been used widely, of assuming the form of distribution of the grade or age group, differs from that here proposed in that it has utilized no check as to the accuracy of the original assumption. The method here described assumes constancy of form of group distribution, not normality of it.

The possession of units, equal to one another in any one of these three senses, would give much greater flexibility in treatment than is possible with a mere ordered series. I have heard the statement "where a difference can be recognized it can be measured" attributed to Thorndike. This may be correct, but I can as yet only heartily subscribe to the proposition that "where a difference between items in a homogeneous series can be recognized it can be measured."

Let us now treat of the bearing of correlation relationship upon scaling. One may ask: Why deal with two variables? Has it not been shown that data with reference to a single variable suffice? Yes, they suffice in the handling of the problems of the single variable; and if a given problem involves two variables this does not lessen the basic significance of the scalings of each of the variables separately. However, there may be short cuts to this task of securing commutative units, and there may be situations in which we have only secondary interest in the units. Thus, for example, if five tests are being used to select salesmen, it may be quite immaterial to us if the units have significance per se. If their multiple correlation with the criterion is satisfactorily high our purpose is accomplished. Of these six items, five tests and criterion salesmanship, we are concerned with securing a scaling of salesmanship only upon the sensed difference basis, and are quite content to let the units of the five tests take care of themselves, provided only that they yield a good correlation with the criterion. We can change the units of the five tests at will, and shall wish to change them so that the multiple correlation of the battery of five with salesmanship will be maximum.

The fewer crude approximations are resorted to, the less of significance will be lost. If we assume a linear regression in a situation where a decidedly culvilinear one obtains, we have decreased thereby the size of our multiple correlation coefficient.

It is however impractical, owing to the statistical difficulties involved, to attempt a multiple correlation calculation of six variables with sundry sorts of curvilinear regression. Even the handling of the three variable curvilinear regression problems is very complex.[9]

We must therefore as a practical matter change our five independent variables into five auxiliary variables yielding linear regression and being therefore more amenable to statistical combination. Thus, keeping the salesmanship units inviolate, let us compress or stretch the units of the first test in some simple systematic manner so as to bring about linear relationships in the table involving the criterion. Theoretically this is not always possible, but experience would suggest that for ordinary situations involving single valued regression lines it can be done. Let us do the same with each of the other independent variables. Then every correlation table involving the criterion will show two linear regression lines. Isserlis has shown[10] that, if any three of the six regression lines found in the three correlation tables of three variables are linear, then all six are linear. Thus we see, by the repeated use of Isserlis' proposition, that if we make the regressions with the criterion linear we shall at the same time make all the regressions in all the intercorrelation tables linear. We shall thus have a final result to which the linear multiple correlation method is entirely applicable. Thus, ordinarily, non-linear regressions between test and criterion constitute no bar to multiple correlation technique.

This method has been proposed so as to bring about a high correlation with the criterion; but is it not likely that it would at the same time result in securing test units of essential merit per se provided the criterion units have such merit?

In the preceding paragraphs we have considered a situation in which the units of the criterion were established and the units of the tests only at our option. In certain theoretical problems the units of the criterion may be of no particular significance except that they give an ordered series. In such case we may change at our option any or all of the units of measurement. Let us say that we desire to change them so as to obtain the simplest possible picture of interrelationships. Surely, if it is possible to do so, it is the part of wisdom to chose a simple picture of mental relationships rather than a complex one.

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[9]See Karl Pearson, On the theory of skew correlation and non-linear regression. *Math. Contrib. to the Theory of Evolution*, Biometric Lab. Pub., Cambridge Univ. Press, 1905. Also: On a general method of determining successive terms in a skew regression line, *Biom.*, 13, 1921.

[10]L. Isserlis, On the partial correlation ratio, *Biom.*, 10, 1914.

The following theorem: "If the arrays of a correlation table are equally variable the correlation surface is normal,"[11] suggests a procedure which, if possible, will result in a great simplification of concept of mental relationships.

One further theorem, stated without proof and following from Narumi's proofs is: "If in a correlation table marginal frequencies are normal and if regressions are linear then the correlation surface is normal." Thus, utilizing Isserlis' theorem and the one just stated, we see that, if it is possible so to change the various units of measurement that all the arrays in the tables involving some one of the measures are equally variable (the number of psychological test intercorrelation tables in which this is quite approximately so without changing any of the units is great), then all of the intercorrelation tables are normal. We reach a picture of mental relationships which is peculiarly simple to comprehend and to treat statistically. Should data be such as to show that this is a possible picture of certain mental relationships, it would be very valuable to attempt to discover further properties of the units pertaining to this system. Should these units turn out to be sense differences, or performance differences, we should be provided with doubly serviceable units.

I have suggested four units of value in mental measurement and have described how each one can be determined: (*a*) the sensed difference unit, (*b*) the variability in performance unit, (*c*) the group variability unit, and (*d*) the unit resulting in the simplest picture of interrelationships. It remains for experimental work to establish whether or not these four are really different. If so, we shall probably need to keep them all and shall need accurate determinations of functional relationships between them.

Whatever the experimental findings are it is certain that, if we can count (and we do so count in our daily affairs) upon a constancy in sensing differences, a constancy in human performances, or a constancy in mental relationship, the possibility of a unit and a science of mental measurement exists.

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[11]This theorem was first stated by me in October, 1922, without proof, in the form of two theorems. (*a*) If the arrays of a correlation table are equally variable the regressions are linear; and (*b*) if arrays are equally variable and regressions linear the correlation surface is normal. Later Seimatsu Narumi, a student in Pearson's Laboratory, proved that both theorems are true, thus leading to the single one given.

THE PSYCHOLOGY OF FEELING OR AFFECTIVE REACTIONS

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Feeling phenomena have always occupied a very peculiar position in psychology. On the one hand, they have been considered as partaking of the most pronounced inner and subjective character. Much more than any other kind of psychological facts have feeling phenomena therefore been considered elusive and unamenable to investigation. On the other hand, from a very early period of modern psychological history the study of feeling phenomena has always involved an emphasis of the so-called external or bodily character of our feeling reactions. For example, upholders of the James-Lange theory of emotions or feelings and their historical predecessors emphatically stress somatic factors if only as expressions of internal mental states. In point of fact this stressing of visceral factors in the study of feelings constitutes in a genuine way one of the actual historical beginnings of a behavioristic attitude in psychology. And so while feeling phenomena have always carried with them the basis for their naturalistic treatment as definite scientific data, still it is probably the bias of internal mentality which has hitherto prevented any naturalistic description of them. To the writer this condition seems intolerable in view of the great importance and prominence of our affective life. Accordingly, the following paper is an attempt to analyze and describe feeling activities as objective scientific facts.

I. *The Nature of Feeling Responses.* Feeling reactions constitute a distinct type of behavior segment in which the responses, though always morphologically overt, do not, however, effect any immediate change in the stimulus objects as in the case of most of the other overt forms of activity. Only one exception is possible, and that is when both the stimulus and the responding individual happen to be the same person. This, however, is clearly a peculiar and non-typical situation. In affective behavior, then, the response or the action to the stimulus is not performed upon the stimulus but upon the acting person himself. In other words, the peculiar fact about feeling responses is that, instead of the person producing some effect upon the stimulus object, the person himself is affected. Emphatically, we are dealing with a genuine adaptational response, although the mode of adaptation is such that the active changes taking place

are localized in the acting person or in his changes of posture or attitude toward the stimulus rather than in the objects or persons acted upon.

The description that we have given implies that a direct overt form of action is performed; that is to say, the stimulus object or adjustment stimulus is immediately present and not absent as in implicit behavior. In implicit activity it is obviously the normal condition that no changes are brought about in the adjustment stimulus or stimulus object; but that is because the latter is absent, and substituted for by some other object. In the case of overt affective behavior, although no effect is produced upon the stimulus, there is nevertheless a direct and immediate contact of the acting person and the adjustment stimulus object.

Moreover, the affective reaction is not a delayed reaction which, after being initiated, is not consummated until a later time, nor is it in any sense action which is inhibited or in some way prevented from occurring. Such notions are implied in the idea that feelings involve a passive state of mind prior to a final and resulting expression which affects persons and objects. Also affective responses do not constitute indifferent functioning of the person, which is attached to or accompanies some other kind of action. No, the affective response is a direct reaction made to its own correlated stimulus. But this is not to deny, however, that very frequently affective behavior influences other actions, as we shall later have ample occasion to observe.

How can we further characterize the intimate nature of affective behavior? To formulate an effective identifying description we must refer to a few members of a large series of differing modes of affective action, each of which depends upon the specific character of its stimulation as well as upon the behavior history of the acting person.

The simplest form of such an affective response consists primarily of an increase or a decrease in the general functioning or activity of the person. The individual may be merely retarded or accelerated in his activity, or he may be said to react positively or attractively, negatively or aversely, to some object. This type of affection represents an apparently unlocalized and ineffective type of behavior which is visibly the farthest removed from producing changes in stimuli objects or persons. The latter simply start up a specialized type of behavior change in the partial or total activities of the person. Possibly this type of reaction can be illustrated by the response of well- or ill-being induced by a bright or a dull day.

Another form of affective responses displays a less marked passivity on the part of the person. This type differs from the kind just treated in that the person is more definitely directed

toward the stimulus object and his action seems more definitely organized with respect to some particular stimulus. That is to say, the reaction system more definitely involves a certain organ or series of organs of both the external skeletal and visceral forms. Such reactions are exemplified by the elementary anticipations or pleasures induced by seeing or smelling food.

Among the more complicated affective responses we may note those in which the person is made depressed or elevated, either in his general behavior attitudes or in some specific action toward an object or person. In these cases the stimuli produce in the person more decided and more positive reactions, stirring him up more definitely and arousing or depressing him more acutely. In these cases the person is more sensitive to the qualities of the stimuli in general and to their particular organization of details. A fairly good example of these reactions is offered us by simple aesthetic responses.

In still more complex situations the effect of the stimulation upon the person will be to increase or decrease his contact with certain things or to arouse his interest in the object or person to the end that he will put himself into a posture for further stimulation in order to be more and more affected, that is, pleased, aroused, depressed or otherwise activated. So intense and complex may the response be that all actions, to other than the affective stimulus, may be inhibited, while the person becomes more and more active, either aroused or depressed, with respect to the stimulus object. In intense situations this direction of activity toward the affective stimuli may fairly amount to a total dissociation of the person. In different words, the person may become manic or stuporic, depending upon whether the stimuli conditions are arousing or depressing. But in no case does the affective response as such involve any actual modification or change in the stimulus object.

Feeling reactions are characterized then primarily by the fact that, even when they are most active and energy-consuming, they do not constitute operations upon the stimuli objects or persons. How is this situation possible? The answer to this question lies in the fact that the affective responses are actions in which the person is above all and essentially responding with internal mechanisms. It is for this reason that the traditional descriptions of affective phenomena have contained the term non-localisability. Now as a matter of fact the affective reactions, as far as the reaction systems of the organism are concerned, are always definitely localisable, for they represent actual responses of the person to their appropriate stimuli. These localisations cannot always of course be observed by the unaffected onlooker. Howsoever "passive" and immediately ineffective feeling responses are, they are still genuine psychological responses and not merely organic states or tones. Feeling reactions

consist of actually organized reaction systems. This warning need be made only with reference to the very simplest of our affective reactions, since it is only those, consisting as they do mainly of visceral mechanisms, which superficially appear as merely conditions of the organism instead of actual direct responses. When we consider the more complicated affective reactions, such as the passion responses,[1] they cannot be described of course as other than specific organized reactions.

And finally we must point out that affective reactions must not be looked upon as indifferently induced by different kinds of objects. This error too is founded upon the belief that feeling reactions are merely biological conditions instead of definite psychological reactions. Were affective responses indifferently aroused by various dissimilar objects they could not be considered as definite psychological phenomena. As a matter of fact affective responses are always aroused by specific stimuli, although we must admit that frequently we experience great difficulty in distinguishing from one another the details of different affective stimuli as well as their corresponding reactions. This condition is, however, exceedingly common in the domain of psychology.

II. Distinction of Feeling Responses from Emotional, Desire and Pain Reactions. Because of the nature of feeling reactions, especially of the simpler and unlocalised type, they are very frequently confused with other types of reactions resembling them in some respect. It is essential, therefore, to distinguish affective reactions from the three kinds mentioned in the heading of this section, which more closely than other forms resemble affective responses.

A. Emotional and Affective Reactions

And first let us distinguish feeling from emotional responses. In the history of psychology the feeling and emotion reactions have always been very much confused, and more especially has this been the case since the emergence of biological conceptions in psychology. The basis for this confusion lies in the fact that emotions very prominently involve organic or visceral reflexes, and so emotional reactions apparently constitute a specific type of affective behavior. This resemblance, which is of course genuine, is, however, merely superficial and entirely unjustifiable. To identify feeling reactions with emotional responses because they both involve visceral reflexes is just as illegitimate as to reduce to one type all the reactions which do not involve visceral mechanisms. Such procedure in both cases results in overlooking all the essential characters of the different types of action. And so, as a matter of fact, when we study emotional and feeling reactions as specific behavior situations, we find very definite differences between them. What are these differences?

Foremost is the fact that in the emotional behavior segment there occurs a total disruption of the person, because a reaction system to some overwhelming stimulus fails to operate. Lacking the appropriate or customary reaction the resulting disturbance of the individual is followed

[1]See classification below.

or accompanied by the operation of many reflex responses of a visceral sort.[2] But let us note that these reflex responses are replacement actions; they are the only type of behavior the person can perform under the peculiar conditions of the disruptive situation; but, unlike feeling reactions, they do not serve as appropriate responses to definitely correlated stimuli. These replacement reflex reactions we must look upon as really biological processes. The person's psychological personality is for a moment, at least, in abeyance. Quite otherwise is the case in actual feeling behavior. Here there is, as we have already indicated, a definite reaction system involved in every case, a reaction system (as we have pointed out) involving visceral factors, but in every instance constituting a definitely organized mode of reaction.

B. Feeling and Desire Responses

Feeling and desire reactions are also confused. The factual basis is that desired objects are very frequently not attainable, and therefore the reaction seems to be one in which no effect is produced upon the stimulus object. Moreover, even when the object is attainable, the desire behavior segment[3] involves two distinct phases. The first phase is an implicit response in the form of an intellectual attitude, wish or thought concerning an absent object which is substituted-for by some other thing.[4] Or it may be an anticipation of possessing a present desirable object.

The second phase is the action of securing the object thus desired. The first phase is probably the more important of the two and should, if it properly bears the name desire, lend its name to the total behavior segment. The second action in the behavior segment is a result and consequent of the first. This statement we make upon the basis of the relative independence of the two actions; for the first may occur without the possibility even of the second transpiring, and in a great number of cases when the second and consequent action *is* possible it may be, and very frequently is, inhibited. Technically speaking, the first action is a precurrent response which calls out, or is a necessary antecedent of, the second reaction. Now, if this description of desire is correct we find here an acceptable basis for distinguishing between desire and feeling reactions. The latter, unlike desire responses, are usually, if not exclusively, the final or completing overt reaction systems in the behavior segments in which they are found.

Additional plausibility is lent our distinction of feeling and desire reactions by the fact that, whether desire responses are precurrent or final, as they would be if they were not connected with another final reaction, they are implicit activities, and thus very distinctly different from the majority at least of feeling reactions.

Feeling responses are also wrongly identified with desire responses through the historic identification of desires with the so-called vegetative functions or appetites. Such identification of very different processes by the use of a common term is greatly to be deplored. For the so-called vegetative functions are clearly immediate visceral reflexes and do not involve the protracted and mediate form of directedness-toward-some-stimulus-object that feeling responses do. The fact that desire reactions are frequently stimulated by visceral reflexes argues more for their difference than for their identity.

Another fact which makes possible the identification of feelings and desires is that in very many cases the thing or person desired is the sort

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[2]Cf. J. R. Kantor, An Attempt Toward a Naturalistic Description of Emotions, *Psychol. Review*, 28, 1921, 19-42, 120-140.

[3]For a description of a behavior segment cf. A Tentative Analysis of the Primary Data of Psychology, *J. of Philos.*, May, 1921, 254-269.

[4]The thing present may of course be a substitute for the same thing possessed.

which produces some affect in us. In other words, desires and feeling reactions are very frequently found to operate together. Possibly it is true that most of our desires are based upon or connected with some kind of feeling reaction.

Just how unsatisfactory this identification of the two types of behavior is we may judge from the fact that the desire reaction may be composed of entirely different components. The desire reaction proper, when it is not connected with a feeling response, which is frequently the case, may not involve any more internal visceral responses than are usually ascribed to non-feeling reactions. This would depend upon the kind of stimulus object desired. Of course when the stimulus object for the desire is unconnected with activities like eating, sex, or (to be more general) does not involve intimate personal or organic conditions, then the reaction is entirely different and is composed of widely divergent elements. We must not overlook the fact that some desire reactions may be considered as practically completely intellectual in character and therefore essentially remote from feeling responses, although here again our intellectual reactions may be connected with feeling responses.

C. Feeling and Pain Reactions

Of the three types of reactions which are incorrectly related to feeling responses, pain reactions are probably more frequently and more intimately identified with feeling activity than either of the other two. No doubt the basis for this situation is contained in the fact that, while pain reactions in common with emotional and desire responses simulate feeling responses in that they do not involve changes in their stimuli objects, pain reactions do involve definite modifications in the person's own condition.

This resemblance between feeling and pain reactions does not, however, in any sense overshadow the decided differences between them. We may point out at once that, in general, pain responses are definitely discriminative or perceptual in character, whereas feeling reactions are primarily immediately adaptational. In other words, pain reactions are precurrent and sometimes anticipatory of further final responses. In this sense pain reactions are passive activities just like any other sort of discriminative action, whether visual, auditory, or any other. All of these discriminative or perceptual responses are passive and thus unproductive of direct changes in their stimuli because they are, on account of their vestigial character, partially implicit actions. On the other hand, however, when the pain reactions do lead to further and more immediately adaptive reactions, that is to say, when their final responses are not inhibited, they bring about very definite changes in their stimuli objects, and in this instance are sharply distinctive from feeling activity.

In considering the relation between pain and feeling reactions we must not be misled by the fact that in the case of pain some actual changes, such as laceration of tissue, take place in the person. For this type of change is very different in at least two ways from the changes in feeling responses. In the first place, the changes in pain reactions constitute the kind of contact with the stimulus object that bears the name of a medium rather than of a response; that is to say, the laceration or other modification of tissues constitutes the mode of stimulation rather than a feature of the response. And in the second place, even the actual changes in the organism performing pain reactions may be located in the superficial anatomical and functional features of the organism and not in the deeper mechanism as is the case with the feeling reactions. Even the older psychologists have pointed out that in pain reactions there is a decided outwardly directed activity involving exteroceptive stimulation and specialized neural pathways such as we find in the case of other perceptual behavior systems; and therefore they have contrasted pain with feeling activities. Moreover, the relative indepen-

dence of visceral changes of even very exaggerated forms of pain responses argues for undoubted difference in the two kinds of activities.

As in the case of the desire reactions the very close interrelations between pain and feeling responses argue for a distinction in type and function of response, since the fact that pain reactions are possibly always stimuli for feeling reactions indicates a very great difference between them.

III. *An Analysis of Feeling Reaction Systems.* As a means of achieving a further understanding of feeling or affective responses it is important to compare their reaction systems, or their unit response factors, with those of non-affective behavior. Our plan is to indicate in what respects affective reaction systems are similar to and dissimilar from non-affective reaction systems, irrespective of any specific differences between affective reaction systems themselves. Not because the latter differences are not exceedingly important and the inevitable results of stimulation by different stimuli objects and conditions do we neglect them, but rather because we find similar differences between the individual members of every type of behavior.

Of the twelve or more unit factors of a reaction system[5] most show marked differences when the affective reactions are compared with the non-affective reaction systems. It is these that we shall emphasize in our brief survey of the unit factors of the reaction system.

(1) So far as the discrimination factor is concerned the feeling reaction system operates precisely like that of any behavior type. In other words, the total reaction or adjustment performed is differential with respect to the qualities of the object, the general nature of the total stimulus object and its relationship to other things (its settings).

(2) Much the same statement may be made with respect to the attention factor of the affective reaction system; since, if we mean by attention the fact of the person changing his response from one stimulus to another, there would hardly be room for any greater difference between affective and non-affective reactions than there would be between any two particular responses in either the affective or the non-affective class.

(3) When we consider the comparative affective factors in the different reaction systems we are impressed of course with the great difference between feeling and non-feeling reaction systems. In the latter, especially when they are definite non-feeling systems, there is indicated only a very slight change in the visceral and other internal mechanisms of the acting person,[6] whereas in affective reaction systems these changes are by far the most prominent features of the whole behavior situation.[7]

(4) Feeling reactions are naturally stimulated by objects and persons in precisely the same way as non-affective responses. In consequence, the

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[5]For a description of a reaction system see article mentioned in Note 3.

[6]It must not be overlooked that every reaction system is itself a unit in a behavior pattern or situation, that is, a unit in some total adjustment of the person, and therefore different reaction systems overlap. We must be understood therefore to be considering ideal or abstract reaction systems.

[7]The critical reader will observe that as a matter of fact the names feeling and non-feeling reaction systems are attached to these specific forms of action.

receptor functions of affective reaction systems or the modes of actual contact with stimulus objects are the same as in other types of behavior. The point to be observed is that, just as in the case of other types of behavior, so here we can observe that the reacting person is primarily in visual or auditory contact with a stimulus object. The same similarity of contact with stimuli is found between affective and non-affective responses whether the stimuli objects or conditions are located within or without the organism.

(5) As we must expect from the general character of affective responses the specific reaction systems will involve an overwhelmingly greater proportion of glandular functioning than is true of non-feeling responses.

(6) Furthermore, it follows also that the sympathetic neural mechanisms will have an exceedingly large component share in the operation of such behavior mechanisms, whereas, in the non-affective response systems, the somatic mechanisms are relatively more prominent.

(7) In both the affective and non-affective reaction systems probably no difference in the functioning of the central neural factor can be observed.

(8) Again, the muscular mechanisms will include a greater prominence by far of the unstriped muscles. Or to put the matter more plainly, the primary muscular action components in affective responses involve the visceral functions, although we should never make the mistake of assuming that the striped muscles are not at all involved in affective reaction systems. At this point we may be reminded again that the reaction system is merely an abstracted action of the organism. Probably the most important indication of the place of striped muscle mechanisms in affective response systems is found in the inclusion of facial grimaces and other facial movements so clearly observable as factors of at least the intense feeling responses. Moreover, who can overlook the large place that the vocal and other linguistic apparatus and functional mechanisms take in affective responses?[8]

IV. Are There Precurrent Feeling Reactions? In our description of feelings we have used as a basis affective reactions which constitute the final factor or act in a behavior segment. That is to say, we have considered only behavior segments which are considered as affective adaptations and which consequently carry the name of affective behavior segments. These segments are affective because, no matter how many other reaction systems they contain of a perceptual or other non-affective sort, their chief function from the standpoint of description is affective. In such behavior segments the feeling factor definitely concludes the adjustment in question.

Now the problem arises whether affective reactions are always final responses, or whether they can serve in other capacities in behavior segments. For example, cannot feeling reactions operate in the same manner as perceptual reactions, namely as precurrent responses to some other and final reaction? To this question we reply that feeling responses, just as well as perceptual reactions, can determine other responses in the same behavior segment. In other words, final reactions in behavior segments may be determined and conditioned by an

[8]Was it not the observation of the close connection between feelings and language which induced writers on language (Wundt for example) to find the origin of language in affective expressions?

affective precurrent response as well as by a knowledge or immediate manipulative action. It must be added, however, that in every case in which an affective response serves as a precurrent reaction it operates along with, and in addition to, a perceptual reaction.

We might illustrate the precurrent functioning of our affective behavior by pointing out that, just as our refusal to buy a picture must be determined by our perceptual appreciation of its nature or character, so the final action of refusing to make the purchase may likewise depend upon our affective reaction to that picture. Whenever the final act of the behavior segment is conditioned by an affective reaction in addition to the perceptual reaction, that behavior segment is of course complicated and becomes more intricate than if the perceptual action alone determined the final response. But this complication, no doubt, represents a common situation in our behavior circumstances.

Not inappropriate is it to add here that, besides the feeling reactions which we have called final and precurrent responses, affective reactions also occupy other positions in behavior segments in such a way that they must be called by a distinctive name. To such reactions we apply the term byplay responses. These byplay activities are incidental feeling reactions which are attached to behavior segments but which do not determine or condition a final response in the segment in which they are found; nor do they themselves constitute a final form of psychological adjustment; they merely operate along with other reactions, a condition made possible by their peculiar morphological character.

V. Are There Implicit Feeling Reactions? Because feeling reactions always comprise decided changes in the person's behavior (a fact which is symbolized in the statement that feeling reactions are definitely overt in their morphological character) the problem arises whether affective responses can be implicit in their functioning.

On the other hand, just because feeling responses are actions upon or in the organism, they can all the more operate without the presence of any original stimulus object, that is, they function through the stimulation of a substitute object. Now if we accept as our criterion of implicit action the operation of a given response to some definite object or person when that stimulus object is not present, then we are impressed by the fact that nothing is more common than the arousal of affective reactions by substitute stimuli.

How frequently do we observe a profound depressive affection produced in us by some person or object which substitutes for, or is the symbol of, some past sorrow or perhaps some grief or disgrace situation! Such a response is a typical implicit reac-

tion. Or again, what is more common than to be pleased or exhilarated by a letter, a friend, a garment or some other substitute stimulus for an adored person toward whom we have acquired affective or feeling responses? The range and complexity of implicit affective activity is revealed to us by the fact that to a considerable extent such behavior constitutes the psychological basis for religious, sex and other fetiches. To consider the bare character and use of fetiches is to appreciate immediately the implicit character of the reactions to them.

We might make the passing observation that implicit feeling reactions simulate much more closely the original overt affective reactions than do the non-affective implicit actions resemble the original overt non-affective responses. Morphologically speaking, the implicit feeling responses are always precisely like the original acts; but there may be a decided variation in intensity. We note, then, that implicit feeling reactions are never symbolized or symbolic in the sense that other types of implicit reactions may be; on the contrary, they are always incipient or vestigial. This latter fact, namely, that implicit affective reactions are very similar to the original overt responses, does naturally bring about a very decided limitation in the range of possible connections of such reactions with stimuli, but it does not in any sense interfere with their occurring at all. In other words, we have a great deal of implicit affective behavior, but none of it can ever be constructive in character. It can only be repetitive, that is, it is the repetition of previous reactions to stimuli, but not the construction of new forms of responses to absent stimuli.

The existence of implicit affective responses solves the problem of affective memory. Often has the question been propounded whether we can have affective memory, since it was supposed that memory consisted of the operation of images which were presumed to be cognitive and symbolic. Now from the standpoint of this present objective description of affective reactions we see that we can very readily recall or act over previous affective behavior.

VI. How Many Kinds of Feelings Are There? In the past history of psychology, when it was the tradition to make psychological facts consist of mental states called awarenesses of things, the whole series of affective reactions was reduced to a very small number. Most frequently it was to two, pleasure and unpleasantness. Sometimes other pairs were added, strain and relaxation, calm and excitement. Possibly we can discover two conditions governing the reduction of all kinds of feelings to so few kinds. One of them was that in the case of cognitive reactions the names of things were made into states of mind, and because there were so many different kinds of things to be aware of a great number of different states of cognitive mind

were enumerated. Now having given the names of the qualities of things and of the things themselves to cognitive states it was presumed, apparently, they could not be further employed as affective terms.

The second basis for the view that there were only a few kinds of feeling responses lies in the fact that the psychology of feeling was developed mainly by ethical writers, who looked upon feelings as the springs of human action. The supreme categories employed by such writers were good and bad; consequently they attempted to find feeling qualities which could be correlated, with this good and bad. These they found in pain and pleasure. When the psychologists took over this conception, they of course did not connect pleasantness and unpleasantness with an ethical condition of goodness and badness, but with a furtherance and hindrance situation within the life of the organism. That is to say, they connected pleasantness with the kind of situations furthering the life activity, and unpleasantness with opposite sorts of situations.[9]

From the standpoint of the actual observations of human behavior we find that as a matter of fact the names historically employed for feeling reactions, such as calm, excitement, strain, pleasantness, unpleasantness, relaxation, etc., may be readily accepted as symbols for particular affective responses, as well as for types of affective reactions. No list, however, that is easily or quickly made can possibly exhaust the specifically different individual and class responses. That list must run into the thousands at least.

Impossible it is to solve the problem of the number of the kinds of feeling reactions that we perform unless we pay attention to at least a great many of the particular affective responses that we make to the great variety of stimuli arousing in us affective or feeling responses. Now the reactions induced by different stimuli conditions differ very materially in their morphological organization and specific psychological function. When we take the attitude that we are studying concrete facts of adaptation to specific objects and conditions we cannot for an instant be satisfied with any sort of limited list of affective activities.

Nor can the idea that there are only a very few kinds of affective processes be justified by asserting that after all only a few distinct affective qualities exist but that these can be mixed with cognitive and other processes to make a large list of complex feelings. For we submit that such an assertion obviously in-

[9]It is without doubt the internal or organic character of affective behavior which gave plausibility to the furtherance-hindrance view of the biological psychologists (Spencer, Bain, etc.). And because of the element of truth in this view it must not be summarily rejected.

volves merely a logical proposition and is not based upon a descriptive analysis of specific behavior or reaction facts. Our suggestion is that even the simplest affective behavior phenomena comprise a large number of individually and typically different actions.[10]

That the proposition to which we have just been objecting is purely logical and not descriptive is evident from the implication that these qualities are always direct opposites. Now the actual study of feeling activities in no sense supports the view that feeling responses all fall upon one or a few two-dimensional scales. An excellent argument against the logical view of the number of feeling types has always been contained in the problem which the upholders of that view had to face in the fact of mixed feelings.

If the feelings are made into simple states which are absolutely opposites, then logically it would appear that you could not have, say, pleasantness and unpleasantness, or strain and relaxation, in the same reaction. Such a logical conclusion of course clashes violently with the actual fact; for in our affective responses, at least sometimes, we have a few or many different kinds of feelings. When we look upon feeling reactions as definite adaptation responses to stimuli there is no fact militating against the inclusion in one behavior segment of both pleasantness and unpleasantness, strain and relief, and other kinds of reaction usually considered as opposed. Moreover, many of our simple feeling responses, each one of which involves only a single affective reaction system, are of such composite qualitative character that we may appropriately apply to them a number of supposedly opposite names. Certainly every one of our complex affective responses, say, to social or aesthetic stimuli, is constituted of a number of different sorts of qualitative reactions. We might describe our responses to aesthetic objects as pleasant with respect to some feature, and unpleasant with respect to some other, calming in some ways, exciting in others, and so on throughout an extended list of affective behavior.

A great obstacle to psychological analysis and description is the poverty of language. This condition is magnified in the case of feeling behavior. Whether this language difficulty is the cause of the intolerable affective classifications or whether it is the effect of it we cannot be sure, but the fact remains that we do not have enough names to describe or to indicate the multiplex and multiform affective responses. In view of the impossibility of estimating even the probable number of distinct feeling reactions we perform, we might suggest a few word symbols which

[10]We assume in this discussion that we may be allowed to interpret the term mental state, employed in the assertion referred to, as meaning responses.

represent unmistakeable simple affective responses and cannot possibly be classified under the heading of pleasantness or unpleasantness. Such names are elated, ravished, delighted, displeased, pleased, horror, disgust, disdain, satisfied, unsatisfied, dissatisfied, longing, libidinous, apprehensive, uneasy, catatonic, anatonic, manic, mirthful, downcast, disturbed, vexed, flustered, contented, etc.

VII. Simple and Complex Feeling Behavior Segments. Already we have indicated that we might look upon some of our affective behavior as comprising only single reaction systems, namely, in the case in which a feeling reaction determines a final response in a non-affective or affective reaction segment. On the other hand, the affective response may itself constitute a behavior segment, and so it may well be that the large number of qualitatively different feeling activities may indifferently comprise either simple reaction systems or behavior segments. In other words, the qualifying terms may apply to both the simpler and the more complex behavior units. Now we want to point out that when the affective phenomena constitute behavior segments they may be classified into two different sorts, namely the simple and the complex.

By simple affective behavior segments we mean the type of behavior in which the stimulus situation is comparatively simple and the response is a correspondingly uninvolved action. The perception of the stimulus merely brings about in the person some sort of immediately final feeling response. Probably the main characteristic of such a reaction is that the response is more subtle than in the more complex behavior segment and approximates more closely to a condition or tone of the person. Consequently, in many cases, the response appears to the observer as a less definite adaptation to a particular object or situation.

The principle of division between the simple and complex behavior segments (let us observe) refers to the simplicity of the total segment and not to the final reaction system which gives the segment its affective character. The point may be made therefore that the final reaction system itself need not necessarily be simple, but may on the contrary be very complex. Of course, no matter how complex the final reaction system is, the total behavior picture presented by the behavior segment in which this reaction system is found will be comparatively simple.

Complex affective behavior segments are characterized by the presence in them of one or more implicit or overt reaction systems besides the affective final reaction. In other words, the entire behavior segment is much more complicated owing to the more involved stimuli conditions. Not only must the individual perceive the stimulus but he must also do more than merely react with internal mechanisms. In listening to music one may

perform merely single behavior segments to the obvious harmonies and single melodies; but, on the other hand, the definitive affective responses may be conditioned by an elaborate analytic and discriminative response to the intricate organization of the nuances constituting the musical stimulus.

Another way in which affective behavior segments are complicated and elaborated is by comprising many affective reaction systems in addition to the other precurrent responses found in the behavior segment. By this statement we do not mean to imply that some behavior segments as a matter of actual count contain only a simple affective reaction system. Rather we should be disposed to think in terms of numbers of different kinds of specific feeling reaction systems.

Further to differentiate between the various types of affective reactions we might consider a number of particular classes under each of the general headings of simple and complex action. These classes are arbitrarily chosen for illustrative purposes and it might be observed that the illustrations of our simple affective behavior segments will indicate a greater prominence of the reaction side of the segment while in the case of the complex behavior segment the illustrations will feature more prominently the stimuli conditions.

A. Simple Affective Reaction Systems

(1) *Expressive Behavior Systems.* These are situations in which the individual's behavior becomes more intense and on the whole he has an arousing feeling response to some specific thing in his surroundings. In other words, provided we have the coordinate reaction systems in our equipment, a sunset, a picture or some person may arouse in us a reaction of heightened tension, of increased affective inclination or of a changed internal and external organic posture, as contrasted with some previous condition. We have purposely considered here the simplest type of affective response, but we must stoutly assert that we are dealing with organized reactions to specific stimuli and not merely with hygienic conditions of the person.

(2) *Depressive Behavior Segments.* In these types of behavior situations the individual is reduced in tone or sensitivity to his surroundings. He does not feel as intensely as he did in some immediate previous condition or in some more remote prior situation. Particular situations such as accidents, the futility of our conduct, or some disappointment may induce in us an immediate withdrawing and slumping feeling. Likewise ugly or disagreeable objects, or things with which we have had some previous unfortunate or undesirable contact, arouse in us feelings of a cataphoric sort.

Every different stimulus situation, as we have so frequently repeated, calls out its own peculiar depressive affective response. In consequence, there are a great many of these depressive behavior segments. Especially important is this statement, for if we carefully heed its significance we may avoid serious error of interpretation. In the first place, we shall not overlook the fact that in every case of affective behavior we are dealing with a concrete response of the person to a stimulus. And in the second place, we may avoid the error of looking upon affective behavior as made up of opposites. For, as we have already indicated, to consider modes of affective behavior as opposites means to look upon them as logical or abstract conditions or kinds of reaction. And so, while it is true that we may arrange

some types of our affective reactions in a two-dimensional scale, this must be considered as a classifactory scheme and not as a descriptive procedure. From the point of view of our actual behavior it can hardly mean anything to put expressive reactions over against depressive responses as opposites.

(3) *Euphoric Behavior Segments.* Quite different from the heightened or lowered tone of the individual's reaction to stimuli are feeling situations in which the person's contact with stimuli of various sorts induces in him reactions described as well-being, reactions which place him at ease, in a condition of calm, and withdraw him from any kind of intensification or depression. A number of different responses involved in sex gratification admirably exemplify these reactions. The English words pleased, satisfied, contented, placid, surfeited and serene may symbolize for us various reactions which can be placed in this class.

(4) *Cacophoric Behavior Segments.* Here we have a type of reaction describable as a lack of calmness or satisfaction. The stimuli disturb the person and remove his tranquility and serenity. Further descriptive terms would be displeased or unpleased, anxious, amazed, apprehensive, and others indicating disturbed relations between certain objects and the reacting person.

Despite the fact that the four kinds of affective responses we have just been describing may well be thought of as different each one from the others, yet it is true no doubt that in many cases a single affective behavior may better be described under two or more headings. Thus, some behavior segments may be elevating, depressing and pleasant all at the same time. The exact description in every case depends of course directly upon the stimuli objects and conditions.

B. Complex Affective Behavior Segments

Not only are the behavior segments of the complex form more complicated because of the presence in them of more and different systems, but also because the individual is in a more active contact with the stimuli objects. What is meant by being more in contact with stimuli we have already indicated in a former section. The responses in these cases are much more describable as adaptations to the stimuli than as effects which the stimuli have produced upon the organism. It would naturally follow that the stimuli conditions in the complex affective behavior segments are correspondingly more definite and complicated things or situations. In general, we might say that such stimuli are found in the more distinctly human situations constituting the milieu of our conduct.

For convenience of illustration we might suggest that such situations constitute what are conveniently referred to as the aesthetic, political, religious, social, sexual, economic, moral and intellectual situations. To illustrate, religious stimuli may produce in us complex feeling reactions which may be named dependent, hopeless, hopeful, awed, sacred, and purified. Aesthetic situations induce reactions of admiration, ravishment, appreciation, transport, etc. Moral and social stimuli are correlated with reactions of sympathy, antipathy, pity, while personal situations may induce love, hate, intolerance, sympathy. Intellectual and practical situations produce reactions of satisfaction, accomplishment, completion and numerous others.

Once more must we be on our guard in the use of our crude and inadequate linguistic tools in psychological description. In the first place, we should note that any name used for an affective reaction does not belong exclusively to a single one of the behavior situations we have mentioned, or to which we have made more general reference. Unfortunately, because of the lack of practice in the analysis of affective responses and the symbolizing of them by words, we cannot be sure that any particular term may not refer to responses which are common to many kinds of situations, even non-affective. For example, the term disgust may be used to describe a feeling

reaction either to intellectual or to aesthetic stimuli. On the other hand, within the range of any one of these situations, say the moral, we shall find a large series of reactions to which we apply the same term. Our main concern is to be certain that in each case we are not overlooking the fact of a specific form of response to some specific stimulus.

Again, it is exceedingly important for us to make sure that each term we use does refer to some specific organized affective reaction to a stimulus and does not symbolize some sort of social conduct other than a feeling response. What we are describing or referring to now are exclusively feeling reactions and not other kinds of behavior which may be very closely attached to, even inseparably associated with, some feeling activity.

A further caution is not to confuse our specific affective responses with large behavior situations which may involve all kinds of behavior segments along with non-psychological facts. For example, we must guard against confusing a particular complex affective behavior segment with, say, the behavior sitituation of purchasing a picture. In this reaction there are many affective reactions, but also we find many others which are distinctly non-affective in character. Again, there may be in that situation a lot of facts which are not even psychological in character, for instance, facts relating to economic values, or aesthetic importance of aesthetic objects,—facts, that is to say, which are not directly involved in psychological action at all.

VIII. Degrees of Intensity of Feeling Responses. In all of our responses we may observe a variation in the intensity of action depending upon the degree of energy expended in their performance. That is to say, whenever we perform a specific response to a stimulus, the general hygienic and other conditions of the person may bring about a more or less intense contact with the stimulus object in question. Such modifications in the physiological or biological status of the person not only make him especially susceptible to certain stimuli but intensify or slacken his reactions. This is of course mainly observable in overt responses in which definite changes are produced in the object. This variation in the intensity with which any action is performed gives us in the case of affective responses a more striking picture of the behavior situation than is true in other kinds of activities. In fact, to a certain extent, when the same affective reaction system operates more or less intensively, there is a superficial appearance of an entirely different kind of behavior picture. We might immediately suggest that this apparent variation is due to the fact that the total affective response is much more intimately related to the person's biological status than is true of other types of behavior.

Such variations in the intensity of operation of affective responses are made possible, then, by the intimate organic character of affective responses. But what are the specific stimuli conditions which bring about these results? Quickly we observe that such changes of intensity depend upon (1) the way the stimulus object is presented, (2) the nature and condition of the stimuli settings, and (3) the length of time that the person has had the affective response included as a part of his behavior equipment.

(1) If the same stimulus object which ordinarily calls out some particular affective reaction is presented suddenly or more gradually than is usually the case, the affective reaction constituting the response to that stimulus may be so much more or so much less intense as to give on the whole a different behavior picture. Especially we must emphasize the point that the response, whether it is a simple action or an affective reaction pattern in a more complex situation, falsely seems to become a qualitatively different kind of activity, merely because of the greater or lesser intensity of the response. How many times has the young man preoccupied with business or other situations been accused by his beloved of not entertaining the same feeling toward her, whereas, in fact, there has been no difference in the quality of his reaction but only in its intensity? Such extreme variations of behavior picture are unique in the case of affective responses.

(2) According to the specific variations in the setting of objects which stimulate affective responses, they will be more or less intense in their operation. And here again, as in the previous case, an apparently different behavior picture is presented by the acting individual, although in actual fact the activity is in its qualitative description the same as in its customary operation. In general, we might indicate that in many cases an individual performing an affective reaction is much more sensitive to variations in the settings of objects than is true of the same individual while performing non-affective responses.

(3) The passage of time between the acquisition of some affective response to an object and the later successive performances of that reaction makes a difference in the intensity of that affective response. The feeling reaction becomes either more intense or less intense as the case may be. This phenomenon is very familiar to us under the heading of adaptation. It has long been known that our activities such as feeling behavior become adapted to situations so that the intensity becomes reduced. Now the other type of modification, namely, the increase in intensity of an affective response, may also occur with the passage of time, during which the acting person has changed organically or physiologically with respect to the particular action mechanisms which operate in this behavior system.

In effect, we have been pointing out in this section that affective reactions involve two sorts of intensity. One of these types constitutes a genuine qualitative difference in the affective reaction system, which displays to us a distinctly observable behavior picture, but which does not constitute a different type of reaction system. We must consider that the stimuli for these somewhat qualitatively different responses are the same. On the other hand, we have a type of intensity difference which represents a quantitative variation in behavior; that is, it is merely done with more energy and seems to indicate no sharp difference in the behavior picture. This second type of intensity of variation is common to both the affective and the non-affective reactions.

IX. The Stimuli for Affective Responses. Investigations of affective behavior indicate that the stimuli for such action must constitute an exceedingly large series of objects, situations and conditions. The truth of this observation appears in the following considerations. In the first place, it is unquestionably

true that because of their character affective phenomena constitute responses to a more varied array of stimuli than is the case with other types of reactions. For is it not true that whatever objects or conditions can serve as stimuli for other kinds of reactions can also be stimuli for affective responses?

In the second place, we must add that the affective stimuli are far more numerous than the combined stimuli for several classes of reactions. For example, affective stimuli are more numerous than those for intellectual reactions, imagination responses, and all the activities of the person involving him in situations more or less detached from organically intimate behavior. It is a matter of common knowledge that most people less rarely perform thinking or problem solving reactions than feeling responses even to the same objects or situations. On the other hand, there are some objects which constitute stimuli peculiar to feeling reactions and do not at all serve to arouse other kinds of action.

Thirdly, because affective behavior segments can be both simple and complex, the number of stimuli within a definitely feeling domain must be larger than in a behavior domain in which no such division of activity is possible. All this means of course that the affective stimuli cover an exceedingly wide range of all the myriad objects and situations that prompt human beings to perform psychological conduct.

By way of enumerating some of the more prominent things serving as affective stimuli we will consider them according to their division into types.

(1) *Objects as Affective Stimuli.* Among the stimuli objects inducing affective responses we may suggest all types of things from the simplest natural objects to the most complex cultural objects, such as art works and social institutions. Furthermore, we find that certain qualities alone of both simple and complex objects can serve to arouse affective reactions. For example, the colors and tastes of fruits or flowers, as well as the whole objects, may induce pleasant, unpleasant or other kinds of feeling reactions. Similarly the specific sounds in music may produce exciting, depressing or other kinds of feeling reactions, as well as the total musical effects. Or possibly specific chords or discords may arouse an individual affectively as well as the entire composition. Again, the intensity and brightness of colors, as well as of tones, may produce varying forms of feeling activity. The same thing is true of the size and intensity of things. Contrasts of objects and of their qualities, such as their colors, and rhythms of all sorts in the visual and auditory objects, are fertile sources of affective stimulation. The same thing may be said for the non-rhythmic changes in the appearance of things and the alternate presence and absence of objects, such as light flicker, etc.

Including persons under this type, we need hardly mention the many qualities such as colors of eyes, slenderness or otherwise of figure, etc., which induce in individuals specific feeling behavior. Here we think at once of the sex factors, although we must not fail to observe that sex stimulation is unique and the affective responses are differential with respect to particular qualities and actions of persons. Almost infinite in number are the affective responses we perform to the human and cultural attributes of persons

in their various capacities and in their interrelationships. Exceedingly prominent are our affective responses to persons which have as their stimuli the linguistic, religious, political, social and intellectual traits of people from different national groups or different strata of the same national groups.

An extremely interesting question arises here, namely, whether on the whole our most complex affective reactions are stimulated by complete objects such as a picture, a person, a statue or a symphony,—in other words, by some large aesthetic or non-aesthetic object,—or whether the several qualities of complex objects can just as well individually be stimuli for complex feeling responses. Possibly there is no definitely fixed correlation, but rather in some cases either the parts or the wholes of objects can be the stimuli for either simple or complex feeling responses.

(2) *Events as Stimuli.* Events of all sorts constitute a very prominent source of stimuli for feeling activity. For the simple affective reactions we find as stimuli the various happenings to physical things, such as injury to a book, or loss of some minor possession. Simple affective reactions are also called out by events involving happenings to persons who are not connected in a personal way with the reacting individual. For instance, an accident involving a stranger will probably call out a simple feeling response on the part of the onlooker rather than a complex one. The more complex and intense affective reactions are usually made to incidents involving persons near to us or disliked by us, and by the more complex affairs of national life, deaths, wars, strikes, famines, etc.

(3) *Conditions as Stimuli.* Whenever the person meets with a difficult or obstructing condition with respect to something that he wants to do, such as some unfavorable weather condition, then there is promptly induced in him some affective reaction. Any kind of hindrance in one's activities, or on the other hand the appearance of conditions favoring such activity, constitutes a stimulus for the operation of feeling reactions. These conditions are constantly present as affective stimuli. We have already had occasion to refer to the general stimulating character of change for affective responses. Now we may point out that the length of time that some event endures may be considered as a stimulus condition for affective behavior. Thus when one is looking at a picture which disappears too soon, or stays too long, the result is an affective response of an unique sort.

Changes in the individual's own condition are a common source of affective stimulation. Such changes may be direct, in the form of hygienic modification, or they may be indirect, through the indulgence or non-indulgence in food, drink, poisons, or by being touched, tickled, or perhaps injured or at least pained (modify tissues).

(4) *Actions as Affective Stimuli.* One of the commonest sources of affective stimuli are the actions of the person himself or of some other. Any successful act may be a stimulus for a pleasant reaction, perhaps, or a calming or exciting one, while unsuccessful action is of course accompanied by unpleasantness and other types of feeling responses. For instance, the successful or unsuccessful manipulation while solving a problem stimulates very decidedly a favorable or unfavorable feeling condition. The recalling of an unpleasant or pleasant situation that the person has participated in likewise produces such reactions, or the inability to remember what one wants to recall, for example, a name.

Especially important among the person's own actions which stimulate affective responses are the general implicit responses constituting the mediate adaptations to all sorts of objects and situations. Not at all negligible as affective stimuli are prior affective reactions of the acting person himself.

Among the more simple action stimuli we may name those induced by our own behavior, namely, eating, dancing, conversing, or reflex action of a sexual or general visceral sort. Here we find a very common and pervasive source of affective stimuli.

Among the affective stimuli constituting other persons' behavior is the action of individuals intimately connected with us, or upon whom we are dependent in various ways. Our affective responses to the acts of such closely related individuals would simulate very closely the responses to our own behavior. For instance, we are fearful for them when they do something harmful to themselves, or we become especially pleased when they do something favorable to their own advantage and social estimation. Again, the actions which disgust us when unrelated individuals perform them would not produce in us such negative affective responses when performed by related persons, especially if such actions arouse no negative affective response when we perform them ourselves.

These suggestions concerning the stimuli for feeling reactions must appear exceedingly fragmentary and incomplete, but they are here set down by way of emphasizing the importance of stimuli in affective conduct. While stimuli as components of behavior segments are always important, in the case of affective reactions especially, the specification of particular stimuli indicates the organized and directed character of feeling behavior.

X. Influences Upon Feeling Reactions. In common with all psychological responses affective reactions are subject to various influences which determine their precise operation aside from the stimuli and their settings. But unlike other sorts of activities the organic character of affective responses makes these influences much more pronounced. Of these influences there are many and all aside from those concerning the person's general physiological and hygienic conditions which we have already discussed in Section VIII.

Our present task, then, is to indicate some of the more prominent influences upon affective action arising from the individual's own reaction circumstances.

(1) To begin with, it is popularly recognized that the type of response a person undergoes immediately before the performance of an affective reaction will distinctly affect its nature. This influence is apparent both when the prior action is affective and when it is non-affective. For instance, if we have just been experiencing a depressing situation our following affective response to some other situation will not be nearly so expressive or so intense as it would have been had the immediately previous experience been a pleasant one. Or some types of previous affective situations may intenstify the following behavior. On the other hand, the same or similar effects upon affective responses may be produced by intellectual preoccupation or by some other contact with non-affective stimuli.

(2) Again, we readily see the determining influence that the individual's general behavior equipment would have on his affective responses. In the first place, the person who has in his action make-up feeling behavior of various kinds is liable to have affective reactions of a particular sort with respect to some specific happening, while the persons lacking those behavior systems in their equipment would have different or possibly no affective responses to that event. To illustrate, an in-

dividual who has been constantly stimulated to be tender with animals will have a decidedly more intense affective reaction when witnessing the maltreatment of some animal than another person who has not built up such feeling traits.

In the second place, the individual who has a large complex of different types of behavior systems in his equipment is more likely to have affective reactions connected with the objects correlated with those reactions than is the individual with a generally meagre set of behavior traits. Most certainly the person having information and art techniques is much more subject to affective reaction with respect to intellectual and aesthetic stimuli than another individual who lacks such techniques. This same fact holds good for all types of behavior equipment coupled with complex affective reactions.

Impossible it is to overemphasize at this point the cultural character of our affective behavior. Owing to the emphasis of the organic and biological factors of affective mechanisms we are prone to overlook the essentially conventional character of most of our feeling responses. But as a matter of fact, although in every reaction we must operate as animal organisms, yet probably ninety percent. of all our reactions are cultural or conventional in character. So far as our affective conduct is concerned we might suggest that since every individual is a product of a specific group, of a particular culturalisation process, his affective responses will inevitably be conditioned by the fact that he acquired his equipment in his particular group. Now since these groups or communities include varying social and intellectual centers, individuals will materially differ in their number and type of aesthetic, moral and other affective responses. Furthermore, since the different culturalising groups are not merely different strata of a given community but large cultural centers in actually different communities we have a basis for national and racial affective traits. The former affective reactions, namely those representing particular individual differences, may be illustrated by the popular notion concerning the physician's stoicism with respect to suffering as contrasted with the rest of the community. National or racial affective traits are represented by such characteristics as the insensitivity of the Romans toward death, compared with the individuals of other national or racial groups. Naturally we do not believe that the cultural differences and characteristics of affective behavior refer to the "expression" of feelings and not to the "feelings" themselves, for obviously upon the basis of our discussion no such differences exist. On the other hand, if our view concerning the cultural character of affective responses is acceptable, it reinforces the description of affective phenomena as concrete responses of the person to specific stimuli.

Briefly to sum up the influencing factors originating in the person's behavior we may call the two main modifying determinants (1) immediate influences, those resulting from behavior immediately performed previous to the affective behavior segment studied, and (2) permanent influences or those resulting from the deep-rooted behavior systems of the individual.

XI. The Conditioning of Feeling Reactions. Of all the reactions which we perform probably the affective responses are most easily conditioned. That is to say, the specific reaction systems can be detached from stimuli in contact with which they were originally developed and reattached to newer and different stimuli objects and conditions. This increased susceptibility to conditions obviously belongs to affective responses because of two of their primary characteristics. First, since affective reaction systems involve primarily the operation of internal mechanisms, they possess a sort of independence of the stimuli. Again, in performing our affective reactions we do not change our stimuli; and in consequence, while every affective response is a specific adaptation to a particular stimulus, the relationship between the responses and stimuli is such that probably they are more subject to be interchanged.

This entire matter may be otherwise stated by suggesting that a peculiarity of affective behavior segments is that they are capable of being interchanged in various ways. That is to say, the stimulus of one may become attached to the responses of the other and *vice versa*. Possibly this fact may be well symbolized by the statement that in the affective behavior segments the response and stimulus factors seem to be less rigidly related than is true in other types of behavior.

This characteristic of conditioning, while probably attaching to all affective behavior segments, is doubtless of more frequent occurrence among the simpler feeling activities. And so while it is very common for us to transfer the simple euphoric responses induced by the brightness of the day to various other stimuli objects and conditions, we probably less frequently similarly transfer any more complex affective actions. Illustrative of the transferred complex feeling response is the case of the person who is stimulated to love some individual and who transfers his affective reactions to objects belonging to this loved person or to others associated with him, even including a whole family, nation or race. Similarily, we carry over our antipathies and enthusiasms from one stimulus object or situation to another.

An individual who becomes enthusiastic about some painting may condition his affective response by connecting it with all the artists of that school although not all the members have the

qualities to which he originally responded. Without doubt much psychopathic behavior consists essentially of the unadaptable conditioning of affective responses.

Affective conditioning processes may be considered to consist of two general types, one of which is not a genuine psychological phenomenon, though it has every superficial appearance of being such. We discuss each in turn briefly.

(1) Pseudo-conditioning processes are those in which there is no actual reattachment of a response to a new stimulus. On the contrary, the affective response just seems to persist and to be indifferently associated with various objects, as if it kept acting on its own momentum. Such pseudo-conditioning situations are exemplified by the actions of the person who allows his antipathies to be carried over from one individual to a whole group, without actually responding to each one of the group separately.

Now just here lies the criterion for spurious and genuine affective conditioning. In genuine conditioning some actual response to a particular stimulus object is reattached to another stimulus object. In spurious conditioning the actual reattaching process is absent, except in appearance. That is to say, there really is no new stimulus to which the old response could be attached. In actual practice the genuineness of the conditioning process is frequently far from being easy to determine. Especially must we be careful in situations in which the transfer is from some particular person to some abstract stimulus such as his profession. Such genuine but ostensibly spurious conditioning processes are common in various forms of insanity. These spurious affective conditionings may be very descriptively designated as feeling illusions or affective maladaptations.

(2) Genuine affective conditioning processes may be differentiated into two sorts, depending upon whether the reattachment is to a new object or to the same object. Any object may ordinarily of course constitute two or more specific stimuli for different reactions. Similarly the same reaction may become detached from one of the two stimuli which a single object or person constitutes, and reattached to the other. Or the reaction need not of course necessarily become detached from the old stimulus, but may be attached to the new stumulus in addition. A striking illustration is the pleasurable feeling induced in an individual by some person in one capacity, say as anatomically attractive, which same reaction may later be attached to the person as a friend, that is, may remain as the response to both.

Genuine affective conditioning, when the stimuli are different objects, is exemplified in the case of a woman transferring a particular affective response to her first child to the later ones, or in the transference of a love reaction from a child who has died to some adopted infant or even to an animal.

XII. The Place of Feeling in Behavior Situations. Such intimate behavior phenomena as feelings must inevitably play a very large part in all of our behavior situations. That is to say, they must, because of their peculiar character, be very pervasive in the various activities of the individual. What we have to point out here is that in addition to having affective reaction systems operate in both affective and non-affective behavior segments, affective processes whether reaction systems, reaction patterns, or complete segments, must and do play a very large part in our behavior situations. By the latter we mean a larger behavior unit than a segment; in other words,

we mean a series of segments. In non-technical language we might say that practically every definitely human type of behavior involves some feeling reactions.

To illustrate, practically every one of our reactions to human beings comprises some sort of affective response, especially if it is a person of the opposite sex and if the stimulus individual is not exceedingly familiar to the reacting person. Also in aesthetic, religious, or sports situations we may distinctly observe a number of more or less complicated feeling activities. Possibly the observation we are making here has been the factual basis for the idea very frequently expressed in psychological literature that the feelings appear to be attributes or tones of other kinds of processes. Certainly, we find feeling activities exceedingly pervasive and also capable of thoroughly interpenetrating with other kinds of activity.

The operation of affective responses in what are essentially non-affective behavior situations may be illustrated by considering a few varieties of complex behavior. To turn first to an aesthetic behavior situation we will consider the constructive action of creating a work of art, a poem or a picture. Such activity we might consider as essentially imaginative in character; in different words, the primary response is the development of an aesthetic conception and its later execution. Such activity also may be designated as a type of social behavior, since the fact of making any particular object, and its execution in a particular kind of medium, are conditioned by a series of group ideas, sanctions and stands, as well as other sorts of specific group institutions. Never can such complicated constructive art behavior be carried out, however, without numerous correlated affective responses.

From the object which is painted or from the person and events about which the poem is created the artist receives many stimuli for the performance of many simple and complex affective reactions. Present here are the calm of admiration, the excitement of enthusiasm for the work or subject, and sexual feeling for the subject the artist is handling.[11] In addition to these there is present the general euphoric response with respect to the progress or success of the accomplishment.

The same or very similar kinds of affective behavior can be traced out in the appreciative or impressive types of affective conduct as well as in the constructive. Doubtless it is the operation of these affective responses, coupled with imagination reactions and others, which constitutes the behavior basis for the empathic or *Einfühlung* phenomena of aesthetic behavior.

[11]The writer hopes it is unneccessary to indicate that this statement has nothing in common with the view that art is the sublimation of sex urges.

Much the same type of combination of affective and other forms of behavior can be traced out in the case of moral action. In the solution of moral problems, for instance, we also find these complex affective components giving character to the total behavior situation no matter how complex the latter may be. An essential difference between the moral and aesthetic situation may be pointed out in that the latter need not be so essentially imaginative in character but may involve more thinking and problem solving activity. Also in other cases, as well as in moral action, we can trace out a large number of memorial and habit activities of various sorts.

Especially important is the observation that the complicated meaning and understanding behavior situations which function to adapt the individual in his most complicated activity are veritably replete with affective elements. The meanings of things, the comprehension of objects and situations, are complicated behavior activities containing a goodly portion of affective reactions. And just as in the case of the aesthetic situation, color, shape, etc. condition the total behavior situation, so do such factors likewise influence the significance of things and persons from a reactional standpoint.

Just how the affective response factors operate in meaning activities may be pointed out in the following manner. Let us assume that, when we have our first contact, or perhaps even some later contact, with some object, this contact results in a pleasant or unpleasant feeling reaction. Now meaning responses are essentially behavior which conditions some other reaction connected with the first one. If the pleasantness or unpleasantness resulting from the contact of the person with some particular object or situation induces a particular kind of response action with respect to that object, which response will later condition other reactions, then we see how the pleasant or unpleasant affective response conditions the development of meaning. This development, of course, is merely preliminary to the future operation of this meaning activity. Our point is that in its development the meaning was greatly influenced by the affective response and the latter constitutes then a permanent factor in that meaning situation.

XIII. Varieties of Feeling Behavior Segments. Certainly by all relevant criteria, and especially because of possessing unique characteristics, affective behavior segments constitute a specific type of psychological activity. But, on the other hand, affective reactions are not thus excluded from participating in the qualities of some of the non-affective reactions. And so we might point out that the feeling behavior segment consists of reflexes, habits, volitional and other types of psychological behavior. Quite definitely do the barriers between different

classes of behavior break down here in the sense that a particular reaction may at the same time be affective as well as volitional or habitual as the case may be. How then may we distinguish between the particular types of affective reactions? Most effectively can we do so by briefly describing each one.

(1) *Affective Reflex Action.* Reflex responses naturally constitute the simplest type of affective behavior. These responses consist of a simple unique reaction system correlated with a particular stimulus object or condition. Such comparatively simple responses depend upon the biological organization of the acting individual.[12] As a matter of detail we might point out that affective reflex reaction systems consist primarily of the operation of visceral mechanisms. Affective reflex actions then appear clearly to be classifiable as members of the general class of reflex responses.

But here we must be on our guard lest we wrongly conclude that, because all affective reaction systems involve predominantly visceral mechanisms, all feeling responses are reflexes. Such is far from being the case. For, as we have abundantly seen, not all affective responses are so simple as to involve merely a single reaction system. Whenever we have a response pattern (more than one reaction system) we must place the reaction in a different class. We shall then have habits, volitions or other sorts of behavior. For example, even a fairly simple sex feeling response must always be connected with social or informational responses in the same behavior unit or segment. Consequently, even such comparatively uninvolved affective reactions must belong to another class of action than reflexes. As we have intimated in a previous section, the question whether we must classify sex feeling reactions as affective at all is determined by the fact that the primary element is the feeling factor rather than the social or informational ones which are also included in the behavior segment.

(2) *Feeling Habit Reactions.* Some of our affective reactions are definitely organized responses which are clearly integrated very closely with specific stimuli. Such an integration process brings it about that the mere appearance of the object constituting the integrative stimulus results in the practically automatic operation of the reaction. Since we call such integrated responses habit reactions, affective behavior of this nature will answer to this description and we will call them feeling habit reactions.

(3) *Affective Volitional Reactions.* By volitional reaction we understand a behavior segment in which the final or definitive response is conditioned, not only by its ordinary stimulus, but in addition by other stimuli objects directly, or indirectly by such additional stimuli objects through reactions of various sorts. Many of our affective responses constitute such multiply determined responses, and therefore they must be placed in the class of volitional reactions. The feeling reaction toward a picture is determined not only by the picture itself but also by the knowledge of its painter, the understanding of its technique, etc.

(4) *Affective Meaning Responses.* We have already indicated above that feeling responses serve to condition the operation of further and final reactions in behavior segments. Now such a conditioning function we call a meaning process. And therefore, the determining affective responses must be called meaning reactions in addition to feeling activity. Here in this case it will be observed that only some reaction systems in a behavior segment, namely the precurrent determining reactions and not the final action, bear this name of affective meaning response; in this sense this class of feeling reactions differs from the others we have been describing. But our point that affective reactions may at the same time belong to other types of behavior is here no less cogent than elsewhere.

[12] Cf. The Psychology of Reflex Action, this JOURNAL, 1922, 19-42.

Similarly our affective responses may be placed in several other categories. We might merely suggest in addition that affective responses may also be considered either as individual responses, in the sense of representing the unique reactions of an individual who has developed in his particular behavior situations an unique type of affective reaction, or the activity may be classified as cultural, that is to say, as we have suggested above, action in which the person has developed part of his behavior equipment because of some group prescription.

XIV. Classification of Feeling Reactions. Impossible it is to classify feeling reactions in any sort of logical or adequate fashion. The various forms are too complicated and too individual in their operation to permit any satisfactory organization; and yet these very conditions which make classification so difficult make it all the more necessary to inject some order into the multiform facts of affective behavior. Even the most unsuccessful attempt at organization would undoubtedly add something to our understanding of feeling responses and supply some measure of descriptive control over them.

When we attempt, however, to plan such a classification we immediately face a serious problem. What criteria from all the possible ones that present themselves shall we adopt as a guide? Here is an incomplete list of them, which we will offer in the form of questions. (1) Temporal condition; or whether the feeling response is momentary or prolonged? (2) Smoothness of action; in other words, does the reaction operate consistently and on a level throughout, or is there a climax? (3) Directness of feeling; is the reaction definitely directed toward some stimulus object or person, or not? (4) Localization; is the feeling reaction unmistakably localized with respect to some organs or some segment of the organism? (5) Is the action simple or complex? (6) Does it involve great activity or not? (7) Does the reaction appear independent of other behavior at the moment or does it accompany some other reaction? (8) Does the affective action represent one of acceleration or of depression and slowing down of the organism? (9) Does the behavior segment involve implicit reactions of other sorts? (10) Is the feeling reaction itself totally implicit? (11) Is the reaction itself of a reflex sort or of a habit type? (12) What particular sort of mixture of behavior is involved in the feeling behavior segment? (13) Is there a preponderance of some one quality in the feeling action, for example, pleasantness or excitedness, etc.? (14) What is the type of stimulus? (15) Is the feeling reaction subtle or crude and violent in its operation? (16) Is the affective behavior suffusing or overwhelming? (17) And, finally, does or does not the reaction represent the operation of some permanent reaction system (behavior trait) in the person's equipment?

Any attempt to classify feeling reactions must obviously be based upon some one or a group of these criteria. Moreover, whatever series of classes we adopt, each class must necessarily include thousands of members. The impossibility then of the classifactory enterprise in the field of the affective responses is manifest. What we shall attempt to do is to select from the more common forms of feeling reactions a few (ten) representative types. For class names we adopt terms in ordinary use, specifying, however, that such names need not be considered as exclusively and irrevocably adhering to the type of behavior with which we connect it. Follows then our attempt at classification.

(1) *Passions.* Here we have a highly organized, usually intense and active form of feeling response which may be momentary or prolonged, overt or implicit. Passions are especially directed toward some particular kind of stimulus object or person. Again, a passion response may involve much implicit behavior of non-affective types such as information, as well as a critical attitude toward the person or object serving as a stimulus. These implicit components of passion responses would comprise of course the anticipatory or precurrent phase of the reaction. To point to examples, we may cite the actions ordinarily referred to as hating, loving, revenging, despising, loathing, jealousy and fits of anger.

(2) *Orgasmic or Irruptive Feelings.* In this class we put (*a*) the feeling reactions ending in a very definite climax after a more or less prolonged operation, and (*b*) the feeling responses which start with a sudden change of behavior. For these responses the stimuli are mainly situations which have abrupt endings or interruptions, or objects which change rapidly. Such actions are performed while hearing music that attains to a climax or which begins with great volumes of sound. Also these reactions occur in watching games, in which there are striking gains and losses, and the person's feelings are thus suddenly initiated or interrupted. Similarly, in observing theatrical performances we may have many sudden irruptions of feeling responses; but probably the best example of orgasmic type of feeling reaction can be found in the sex reactions. For the orgasmic and irruptive type of feeling reactions we cannot find very definite word symbols in our language, but the expressions striking disappointment, overwhelming surprise, sudden indignation suggest the kind of behavior under discussion. In contrast to the passions, the orgasmic or irruptive feeling responses are more turbulent, fitful and less organized or well-knit, and in this respect they are closer to the emotions than any other type of feeling behavior.

(3) *Affective Sentiment Reactions.* Sentiment reactions represent very highly organized activities which typify acquiesence or readiness to do certain things or to refuse to do them. They are usually exceedingly complex and very intimately connected with complicated knowing and informational behavior. They are also for the most part developed and function as cultural or social modes of action. In each case sentiments are unmistakably directed toward some particular kind of stimulus object or situation. Probably most frequently the stimuli for sentiment responses are social institutions of the group behavior variety. Names symbolizing sentiment reactions are shyness, altruism, patriotism, modesty, shame, decency, indecency, cleanliness, charitableness, etc.[13]

(4) *Affective Dispositions.* The main characteristic of disposition reactions is the fact that the feeling activity when it occurs in response to its

[13]None of these names refers to social conduct, but only to the particular affective responses of persons, but these affective responses may be social in character.

particular stimulus represents a practically permanent behavior factor of the person's equipment. Thus the dispositions may be considered to a certain extent as giving character to the person. The stimuli for these reactions may be objects, situations, and persons. Such dispositions are tenderness, hopefulness, optimism, pessimism, etc.

(5) *Affective Attitudes.* These reactions are contrasted with dispositions in representing an action of the moment and do not necessarily reach back to definite elements in the personality behavior equipment. By feeling attitudes we mean more or less prolonged responses with respect to particular stimuli which are usually immediately present. They constitute reactions, then, that are organized with respect to some particular kind of situation object or person which is usually present. These reactions may be exceedingly intense, such as some extreme happiness over some situation or a hopefulness concerning some event. Very good examples of affective attitudes may be found in aesthetic situations; to be enraptured in a work, inspired or repelled, represents such behavior and in ethical situations to be disconcerted or astonished by some one's action. When these expressions represent definite affective responses they may be considered as typical feeling attitudes.

(6) *Affective Moods.* Moods constitute feeling reactions consisting mainly of a simple type of slightly organized conduct. Qualitatively the mood reactions are in effect conditions of depression or elevation of the person, but these depressions and elevations are none the less organized and directed towards a stimulus object or condition, since they comprise a number of varieties differing with the arousing conditions. Because of the simple character of these reactions, however, they have for the most part been considered as merely conditions which color other types of action. Much plausibility attends this observation, since the comparative simplicity of mood responses makes them especially subject to conditioning. In consequence, they seem to belong indifferently to various kinds of behavior situations. Also moods appear as not directed and autonomous responses because probably for the most part they are stimulated by hygienic conditions of the person himself. Moods may be symbolized by the popular color-terms, blues, browns and rosy moods.

(7) *Continuous Feeling Responses.* A very prominent type of feeling reaction we are constantly performing are the affective responses of an unlocalized sort which accompany other actions and which to a considerable extent may be said to have their stimuli in other actions of the person. In these cases we have definite pleased, displeased, satisfied or unsatisfied, disappointed, apprehensive or other affective responses which parallel and accompany other actions. The essential characteristic of these continuous feeling reactions is that they are not independent responses; that is, they depend upon and are closely connected with other reactions. To illustrate the operation of these actions we might indicate that during a conversation we may have definite pleasurable, excitable or other sorts of feeling responses, depending upon the nature of the conversation and the sort of person with whom we are conversing. Such forms of feeling reaction may frequently amount to a suffusion of the individual with pleasantness or other sorts of feeling. Continuous feeling reactions are naturally not connected with objects as stimuli, but rather with series of objects or continuing action (conversation) and in general with events and conditions.

(8) *Diffuse Feelings.* In this class we place a large series of complex organized reactions the members of which have a longer or shorter duration in their specific situations. Although the responses of diffuse feeling segments are directly related to some stimulus object or condition, still the segment as a whole appears to feature the response indefinitely more than the stimuli objects and conditions. In other words, the reference to the stimuli objects is not very marked on the surface, especially when the total segment is of considerable duration.

Diffuse feelings usually constitute responses of a serious or important nature. They operate in situations which greatly affect the person. It is for this reason that the response appears so much more in evidence than the stimulus. Thus the responses are diffuse and apparently undirected. We might point out, too, that the most typical of these diffuse feelings have as their stimuli past or future situations or present conditions which are inevitable and cannot be controlled by the individual. This fact adds also to the diffuseness of these reactions. Well symbolized can such behavior be by the responses we call regretfulness, remorse, gladness, elation, dread, anxiety, ecstasy and resignedness.

The diffuse feelings may be differentiated from the continuous feelings chiefly by the fact that the latter are not independent responses but are always related to some other kind of response. Again, the continuous reactions represent slighter forms of behavior.

(9) *Localized Feeling.* By localized feeling we mean definite forms of direct feeling responses to stimuli which are immediately present and in close contact with the individual. These stimuli, however, may be simple or complex, with a simple or complex response on the reaction side. Here we refer to the pleasantness or calming affect produced by touching, stroking or hurting the skin, or the kind of feelings that accompany such actions as eating in which we can localize a pleasant or unpleasant feeling on the tongue.

(10) *Tender Affective Responses.* Classed here is a type of highly organized feeling directed toward a person or some other living thing. The specific qualities of these responses are correlated with these types of stimuli. That is to say, these reactions involve implicit responses to the suffering and the joy of the persons or other living things that constitute the stimuli. To a certain extent the person reads himself into the situation of the stimulus individual, and imagines his trials and triumphs, and himself suffers accordingly. For names of these reactions we might use the terms sympathy, meltingness, pity, forebearance, etc.

Specific differences between the tender feeling responses depend upon the character of the person and upon the relationship of the stimulus person to the reacting individual. Of course in every case we are ruling out any sort of intellectual compassion or pity and insisting that the reaction when it occurs must answer to the particular characteristics we have specified as belonging to the feeling reaction.

Summary

In this paper we have attempted to suggest a naturalistic description of feeling phenomena. By this we mean that we describe feeling reactions as actual ways in which the person adapts himself to his affective stimuli. Upon analysis we have found that a feeling reaction system consists of adaptations which involve primarily the visceral mechanisms, facial movements and such external skeletal mechanisms as do not produce any direct effect upon the object. As a consequence the most characteristic feature of affective adaptations is that they constitute changes or effects in the person himself, and not effects in or upon the stimuli.

Striking superficial similarities appear to align affective responses with emotional, desire and pain reactions. In consequence, feeling responses are very frequently confused with one

or more of the latter. Because of this situation we have essayed a differentiation of the three latter forms of behavior from the affective reactions.

Assuming that affective phenomena are concrete psychological adaptations of a specific sort, it follows that because of their intimate personal character and frequency of occurrence there must be a great number of qualitatively different types. These qualitative differences constitute essentially different reactional characteristics which are not reducible to any sort of logical abstractions.

Concerning the stimuli for affective reactions we find that these are extremely numerous and very frequently are conditions, events, modifications of things, acts and qualities rather than composite objects.

Among some other results of our study is the observation that the typical affective reactions partake of behavior characteristics of other psychological phenomena. Accordingly, we find that affective behavior comprises reflex, habit and volitional reactions besides a great many varieties of cultural conduct.

In lieu of the impossible task of classifying feeling behavior we have selected from the more familiar types of affective reactions ten classes which we have attempted to distinguish from one another. As names of these classes we have retained whenever possible the terms usually connected with them in so far as this was practicable.

BOOK REVIEW

Behaviorism and Psychology. By A. A. Roback. University Bookstore, Inc.: Cambridge, Mass. 1923. Pp. 284.

In the present volume, Dr. Roback sets himself the task of evaluating Behaviorism as a point of view in psychology. He handles the problem under the following main headings: (1) history and exposition; (2) criticism of the principles of behaviorism; (3) practical applications of behavior in such fields as ethics, law, medicine, religion and society; and (4) the future of behaviorism and psychology. The book closes with a chart which presents twenty-one varieties of behaviorism and which lists some forty individuals. The varieties are grouped into four main classes: pre-behaviorism, behaviorism proper, psycho-behaviorism, and nominal behaviorism. The subdivisions of behaviorism proper are given: (1) as structural (gross, Holt; physiological, Watson and Weiss; neurological, S. B. Russell, Meyer, et al.; formal, Tolman); and (2) as functional (bio-sociological, Parmelee; physico-social, G. DeLaguna; philosophical, Bode, Perry, et al.; and dynamic, Holt, Kempf and Kantor). The organization of the chart is such that practically every member of the American Psychological Association is eligible to a place in it! The omissions which strike me as the most conspicuous are those of Angell (as a pre-behaviorist), Judd (in connection with motor theories), Thorndike, and Woodworth.

The historical section relates behaviorism to materialism and realism in philosophy, to the views of Comte and Cournot, and to other more recent philosophical and psychological writings. McDougall is given credit for first defining psychology behavioristically, although the author points out that McDougall has never carried out the definition. The author's presentation in this section and throughout the book is too polemical and sketchy to be a good example either of historical analysis or of dispassionate criticism.

In putting the case against behaviorism, Roback emphasizes the following criticisms, which I shall list with what I conceive to be their possible answers.

Behaviorists criticize the introspective psychologists for their nebulous theories and yet they present hypotheses of the same character,—as witness Watson's treatment of affection and the image and the physiological constructions of Meyer, Russell and others. And yet, except for the philosophers, the behaviorists have avoided the issues and objections raised by the opposite side.

This criticism can hardly affect the value of the behavioristic position. Undoubtedly there have been extravagances and mistakes as there are in all revolts; but the behaviorists have done well to let most of the talking be done by their opponents. Most of this talking has been done with the purpose of showing that consciousness does exist and that the behavioristic position involves that assumption. (Lovejoy is perhaps the most glaring example.) If consciousness exists, then psychologists should study it because no other science does so. Now I gather, from what Watson (whom Roback calls an "enfant terrible" and other undignified names) has written, that even he would not deny the existence of consciousness. To do so would be to enter upon the discussion of philosophical problems, and this the behaviorist refuses to do. What Watson does do is to doubt the possibility of making a scientific study of what others call "consciousness." My own views would be in harmony with this position to this extent:

although I believe that a scientific study can be made of conscious states' as has been done by the introspective psychologists, I do not think that these studies bulk large relatively in the history of scientific psychology or that those studies which have been made are of particular value. This last point is, of course, the expression of personal taste, but I suspect that a similarity of such tastes is at the basis of much of the behaviorists' activities. When I say that introspective studies do not bulk large in scientific psychology, I mean that few studies have been devoted to the description and analysis of consciousness, in spite of the fact that practically all studies have used the terminology which descends from the Greek philosophers. Many studies can be interpreted in either of two ways. Thus studies of sensory processes may be regarded as of value because they indicate the nature of conscious qualities or because they get at certain sensory conditions of behavior. So far as psychologists take the former view, so far are they subjectivists tending to reduce the science to what Yerkes has called a field of "pure psychics." Now I submit that a world of "pure psychics" may be interesting, but that it is of value only for aesthetic contemplation. It is decidedly worth while from the human standpoint to assume that one's sweetheart is not an "unconscious" automaton; but that is partly because such interpretations are intimately bound up with behavior, and partly because of the aesthetic satisfaction involved in such theories.

I now come to a second criticism stressed by Roback. The behaviorists have no experimentation to show on lines divergent from introspection (p. 71). Undoubtedly the author has in mind some other meaning than that which would ordinarily attach to such a statement, but I can not discover it from the context. All of the studies in animal psychology, infant psychology, and mental tests, and most of the studies of learning and memory, have been accomplished on lines divergent from introspection in the sense that this method has not been used. Many of these studies have been made by men with behavioristic leanings. In many cases the authors have felt it necessary to *add subjective interpretations to their observations in order to convince themselves that they are working in psychology*, but such interpretations are irrelevant to the nature and scientific character of the experiments and as such are definitely opposed by behaviorism.

A third criticism of behaviorism which the author urges is that the term behavior is too indefinite to serve as a name for the subject-matter of psychology. Even the physical sciences speak of the 'behavior' of organic and inorganic material, while physiology and the biological sciences are largely preoccupied with the 'behavior' of animals. An examination of the studies made by behaviorists shows that even the distinction between "whole and part" behavior is not adhered to, while it also shows that these students are unable to describe responses correctly because they ignore the intentions of the subject which alone serve to distinguish many responses one from the other.

The main issue raised here seems to me a fruitless one. Psychologists need concern themselves no more about the definition of their science than do the physicists, chemists, biologists, and others with the definitions of their respective fields. Those who are devoted to clear-cut definitions deductively arrived at are horrified of course by the statement that psychology is the science studied by psychologists, that its subject-matter is very diverse and is determined largely by academic and other social conditions, but that its goal is the scientific understanding of human nature in which it emphasizes both "consciousness" and "behavior." Numerous philosophical questions can be asked about such a definition, as they can be about the terms employed by the other sciences. Indeed who can discourage philosophic meddling with any question? And yet I conceive these problems to be no more the concern of the psychologist than are similar ones the concern of the physicist. The behaviorists will in time be able to offer a clearer picture of what psychological behavior is in terms of the

behavior which has been studied by men known as psychologists. Any definitions or hypotheses proposed in advance of this historically accumulated information must be justified by their significance for the control of observational procedures.

The critics of behaviorism, including Dr. Roback, make much of the philosophical implications of that point of view and chide the behaviorist for not looking better to his philosophical groundwork. The behaviorist, I believe, intends to let the philosopher make any examinations and speculations to which he may be inclined, while the behaviorist applies himself to his experiments like any other good scientist. (And yet Dr. Roback says (p. 32) that behaviorism is merely a philosophical attitude applied to the subject-matter of psychology!) Let us not scold the behaviorist for his indifference to philosophical problems. Let us rather give thanks that some psychologists are so innocent in philosophical matters that they may attack their problems with the same single-mindedness that characterizes the approaches made by other scientists upon their problems. I do not mean to say that the behaviorist is the only psychologist who tries to keep away from philosophical questions; but I do insist that he succeeds better than the structuralist, if for no other reason than because of the novelty of his terminology. I think, when Watson claims that he doesn't know what the terms 'imagery', 'consciousness' and 'affection' mean, that he is influenced by the long philosophical controversies summed up in each term. It seems impossible to define the classical terms in psychology in such a way as to insure acceptance, and this situation seems certain to persist just so long as psychologists are steeped in the philosophical history of these terms. It would be psychologically unexpected if men of markedly different philosophical tastes and training should attach the same meanings to the terms which they use. Terms like sensation, affection, image, emotion, etc., will always be productive of verbal controversies for this reason, and progress demands their elimination. If we are to study consciousness, let us get some terms which are new and which do not invite differences of opinion by virtue of the various meanings attached to them since the days of Aristotle. The behaviorist himself has still some housecleaning to do. Even Watson uses such terms as instinct and emotion! One needs only to read through the recent discussions to realize that it has become impossible to secure agreement on the meaning even of the term *instinct*. Let us give up this term also, then, rather than spend precious energy in verbal battles. Agreement can be reached concerning the observable phenomenon, and this can be given some colorless name such as *alpha* behavior. Behavior which has a different genetic history in the individual can then be termed *beta* behavior, and so habit and instinct as terms may be eliminated. If such action seems necessary in *behaviorism*, how much more imperative is it for the structuralists to free themselves from the Idols of the Market-Place, which have obscured genuine scientific issues since philosophy began! A good example has been set for all of us by the mental testers. Their success is in proportion to their avoidance of subjective terms which would draw them away from the real business of the day. One term, however, has threatened to hold up experimental work because of its subjective significance and philosophical history, the term 'general intelligence.' These men, however, have been wise enough to make but little use of the term and to emphasize the concrete attack upon their problems, although every now and then a pathetic wail goes up, from some one who is hurried along by the procession, to the effect that nobody knows *what* is being tested. And of course no one does know, in subjective terms, and few care! I venture the assertion that most of the opposition to the use of tests which is current today is due to the ambiguities of the term *general intelligence*.

Dr. Roback has two other criticisms, to whose consideration we must turn. The first of these is that the behaviorists refuse to use the introspective method and hence do not have access to all of the data. This I be-

lieve is an important systematic question, quite irrespective of whether introspection be thought to give information concerning consciousness or not. Roback overlooks the fact that Watson would not exclude verbal responses and the testimony of the subject from the methods of psychological investigation. Watson, writing in the symposium on thinking, explicitly provides for what most psychologists have termed introspection, but he has never developed the implications of such an admission. Does he not thus let in at the back door all of the subjective problems which he has so unceremoniously bundled out at the front? The position can be so interpreted, but no one who knows Watson will believe that to be his intention! There is here an important phase of his position which he should develop, not only in the interests of behaviorism but also, what is much more important, in the interests of unity within the psychological field. My impression is that the subjectivist and objectivist can get together only when the former will accept a terminology which will discourage the philosopher and yet which will refer to those "experiences" which all can observe. My suggestion intends to have little in common with the earlier attempts by Bethe and others to establish an objective nomenclature, inasmuch as the present suggestion seeks new terms for subjective material and not for objective material only.

We now come to the final argument against behaviorism which we are to consider. It may be formulated as follows. Inasmuch as ethics, jurisprudence, medicine, religion and the social sciences are written in terms of subjectivism, behaviorism is incompatible with them. If psychology is to be conceived in terms of behavior, it must lose all contact with these fields. Dr. Roback must attach primary importance to this criticism, for he devotes one-fourth of his book to its exposition. Again I find myself unable to agree with the author. In the first place I wish to dissent most emphatically from the assumption that any science should so regulate its conduct as to satisfy some other field. The business of the psychologists is to define their terms and to conduct their investigations in the manner best suited to solve their problems. This is the fundamental requirement. If psychology gets the facts, other disciplines which rely upon it will be only too ready to incorporate the material. Physics does not conduct its work so that the results will be suited to engineering. Rather engineering takes whatever the physicist can offer which is relevant to its own problems. So biologists seek for the facts in their field in the best way possible, and medicine must use the material or go without. One need not read psychologists a lecture upon this moral! If they find themselves talking in the same vein as that adopted in various related disciplines, the situation is only the result of the incorporation into those disciplines of the only psychology offered them. Psychologists will set their own pace and study their own problems in the ways most likely to bring reliable results. And we need not fear that the social sciences and other disciplines will reject the data because of their newness or because of technical terminology. What is wanted in these fields are the facts about human nature, and it is the psychologist's problem to furnish this material in an unbiased and scientific manner. I am therefore much more optimistic concerning the future of behaviorism than is Dr. Roback, because I feel that this point of view keeps closer to the verifiable phenomena of human nature than does "mentalism."

The University of Kansas WALTER S. HUNTER

NOTES

Relearning after Forty Six Years

On my tenth birthday, in January 1877, I recited without mistake Milton's *Hymn on the Morning of Christ's Nativity*, a poem of twenty-seven eight-lined stanzas. I recall nothing of my method of learning, except that it was piecemeal; I had learned a certain number of stanzas (three?) as a week-end task for what seem in retrospect to have been many school weeks.

As it happens, I can say with assurance that I had never reread the poem until March of the present year, when I undertook the experiment which is the subject of this Note. It occurred to me that it might be worth while to see how easily, after an interval of forty-six years, I could relearn the hymn. I must premise that I am accustomed to learning by heart. Also, when I looked about for a book to learn from, I was lucky enough to find on my shelves my old school edition of Milton, so that I could count on the revival of memories of place.

The record of relearning is as follows. The abbreviation a. m. means the first thing in the morning, just after breakfast, and p. m. means the last thing at night, just before going to bed. All readings recorded are complete readings.

Saturday, March 17, p. m. (1) One silent reading. Notes made afterward.
(2) One loud reading.

The first reading could hardly have had its full value, since I was interested to see how much of the poem was familiar, and made mental notes as I went along. I recognized verbally three lines of Stanza XX.; two of XIV. and XIX.; and one of Stanzas II., V., VIII., XXIII. These eleven lines had, for one reason or another, become a permanent part of my mental furniture. I found a vague familiarity in Stanzas I. to V., VIII., XI., XXVII. Certain stanzas—VII., X., XVI., XXVI.—struck me as definitely strange. The rest were blank: many phrases and word-groups fitted together more smoothly and easily than would be natural at the first reading of a poem; but there was no positive recognition, however vague. My general impression was that of discursiveness; the stanzas seemed to be linked rather by their author's current associations than by logical plan; and I was a little afraid that I had undertaken a burdensome task.

March 18, a. m. (3) One silent reading.
March 18, p. m. (4) One silent reading.
(5) One loud reading.
March 19, a. m. (6) One silent reading.
March 19, p. m. (7) One silent reading.
(8) One loud reading.
March 20, a. m. (9) One silent reading.
March 20, p. m. (10) One silent reading.
(11) One loud reading (recitation with promptings).

Hitherto I had read slowly and carefully (the silent reading took about 9 min., the loud reading a little longer), with keen attention to words; I had never read any line or phrase twice over, but the reading was slow enough to permit me to note, *e.g.*, the shift of tense in VI., the shift of

gender in XIV., the repetition of *in vain* in XXII., XXIII., XXIV. After the tenth reading I felt that I had some grasp of the whole; and at the eleventh I tried to recite,—covering the printed page with a card, and exposing the lines one by one at the least check of memory. I have no record, but I read a very great deal more than I recited.

March 21, a. m. (12) One silent reading.
March 21, p. m. (13) One loud reading (recitation with promptings).
(14) One loud reading (recitation with promptings).
(15) One recitation.

The thirteenth reading showed marked improvement; and the fourteenth went so readily that I was tempted to try a recitation without book. So far as I know, the recitation was correct.

March 22, a. m. (16) One silent reading.
March 22, afternoon (17) Recitation to a colleague.

This recitation—with one exception, which can hardly be accounted, in the ordinary sense, an error—was correct. In IX. I gave *warbling* for *warbl'd*. After the recitation was done, the friend who had been good enough to 'hear' me remarked on the mistake, and I bade him look again at his text; I was very sure that he had himself misread. Only when I took the book into my own hands was I convinced that, all along, I had read the wrong word for the right. Whether I had seen the apostrophe as the dot of the *i*; whether I was 'attracted' by the *answering* of the next line; whether my nervous system inhibited the double rhymes *warbl'd voice* and *stringed noise*; I do not know. I am pretty sure, however, that such a misreading could occur only in the case of material already once learned. And indeed, now that I have recovered the poem, the whole seems so absolutely familiar that, were it not for the notes jotted down after the first reading of March 17, I could not believe what I have said about its original blankness.

E. B. T.

A Demonstrational Qualitative Series

In order to show, in a striking manner, how from a mixed medley of colors may be derived a qualitatively regular series we have prepared the following demonstrational apparatus. A frame about 1 x 5 ft. with a solid back carries three sets of colors. One set is fastened to the back of the frame. The other two are in slides with windows. The two slides move independently of each other and of the back. The fixed series is composed of rectangles of color in the order (1) YG; (2) R; (3) V; (4) OR; (5) G; (6) RO; (7) dark BG; (8) rO; (9) B; (10) yO; (11) RV; (12) YO; (13) verdigris; (14) OY; (15) P; (16) Y. It will be noticed that the odd numbers in the series vary irregularly in quality, but that the even numbers form a series from R through degrees of O to pure Y. The two slides carry colors and blank windows alternately. In the front slide the colors form a R-O-Y series. In the back slide the colors are duplicates of those in the front slide, but are arranged out of order.

In the first position for demonstration the *O* sees the colors of the front slide and, through the windows of both slides, the odd-numbered members of the fixed set. This set of 17 colors has 9 members in a regular order; but between them the other 8 members are chaotic. In the second position, the back slide is drawn along until its windows are covered by the colors in the front slide, and its colors cover the odd, irregular colors of the fixed series. The *O* now sees 17 colors (9 from the front, 8 from the back slide), all of which are members of a R-O-Y series; but they are out of order. In

the third position, the front slide is moved until its colors cover the colors in the back slide, and the windows of both slides expose the regular series of the even numbered members of the fixed set. The *O* now sees *an orderly arrangement of 17 colors*, starting with R and passing by regular steps of increasing yellowness to pure Y. There are stops to prevent drawing the slides too far. This feature of the demonstration assures coincidence of the windows and colors in the several positions.

The colors of the odd numbers in the fixed set are of colored paper. All the members of the R-O-Y series are papers covered with ordinary wall-kalsomine, a form of pigment which is easily mixed, easily put on, gives a dead surface, and is sufficiently permanent. By a few empirical trials we determined what geometrical series of increments of R should be added to a constant amount of Y to give the proper qualitative steps.

H. G. Bishop

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A Method of Obtaining Records of Constant Blood Pressure

In a current experiment on emotional reactions, it was desirable to have some means of taking systolic blood-pressure changes both quickly and easily. Also a method where the pressure-change could be closely followed over a space of some 5 min. was sought. Reference was found to Kolls'[1] method of taking constant records; but this involves a rather complicated apparatus which would have to be specially built.

The laboratory equipment included a "Tycos" sphygmomanometer of the office type; a compressed air tank and a hand pump[2]; and a "Bowles Patent" blood-pressure bracelet stethoscope. The air-pressure tank was substituted for the small hand-bulb as a method of inflating the armband, while the systolic pressure was taken stethoscopically. This method was found to give a quick and easy reading of either systolic or diastolic pressure. The main advantage, however, is that a series of readings which are close enough together to form practically a continuous record of changes in systolic pressure during a period of 5 to 10 min. may be easily taken.

Say, for example, that the blood pressure is 120 mm., and that owing to emotional excitement it starts mounting; by a slight manipulation of the air-tank outlet-valve the pressure can be raised in the arm-band to follow up the rise in blood pressure. With a decreasing blood pressure the closing of the tank outlet-valve and the opening of the escape-valve will allow a fall in the arm-band pressure. Both the rise and the fall may be followed very closely. This method has been found to give records of form of the systolic blood pressures in a very satisfactory manner.

University of Minnesota Carney Landis

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[1]A. C. Kolls. Continuous blood pressure tracings in man, *Jour. of Phar. and Exp. Theraputics*, 15, 1920, 433–41.

[2]Supplied by the Standard Scientific Co., E200, Catalogue 3, 1920.